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ICE CAVES AND THE CAUSES OF SUBTERRANEAN ICE.*

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I. DESCRIPTIVE AND EXPLANATORY.

Terminology.—The term "ice cave" refers to a rock cavern containing ice. It does not refer to a hole or tunnel, cut by the hand of man into a glacier, such as those one sees at Grindelwald or Chamonix. I make this statement, because on mentioning ice caves in conversation I have repeatedly been asked whether I meant those tunnels. The Germans use a term similar to ours, *Eishöhle*, while the French and Swiss use the word *glacière*, the feminine of

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glacier, which they also apply to artificial ice houses, speaking of ice caves as *glacières naturelles*, and of ice houses as *glacières artificielles*. The term *glacière* seems to me the most accurate in use in any language, and, if it were not too late to do so, it would be an advantage to use it, especially as we have adopted the term *glacier* from the same part of the world. In my opinion, the term "ice cave" should especially apply to the hollows in the ice at the lower end of glaciers, whence the glacier waters make their exit. The Hon. George N. Curzon uses the term, apparently in this sense, in a sketch published in the *Geographical Journal* in connection with his recent journey to the Pamirs, and in the account in the same publication of the visit of the English Training Squadron to Spitzbergen, in 1895, the term was applied to a natural tunnel found in a glacier.

Glaciers.—Ice enduring all the year round is found in three principal forms: glaciers, ice gorges and ice caves. Glaciers are formed, to speak in general terms, from the winter snows which have fallen from the skies, and, by their own weight, and melting and regelation, have accumulated into a mass of ice, causing the phenomenon known as a glacier. Glaciers depend, therefore, practically on the annual snowfall and a low temperature for their existence.

Ice Gorges.—Ice gorges or ice gullies show kinship to both glaciers and ice caves. They occur in fissures or ravines, at an altitude below the general snow line of the district, where the winter snow is sufficiently protected from the sun to endure either as snow or ice through the summer months. In some cases the surface ice and snow melts away, while underneath boulders, at the bottom of fissures, ice still remains. For instance, I have found lumps of ice in King's Ravine, on Mount Adams, in the White Mountains, late in September, among the big boulders which form such a trying path for the traveller. In such gullies, however, we have something resembling rather miniature glaciers than ice caves, in that gullies receive the accretion to their ice directly from the winter snows. Some of the reasons for their endurance are similar, however, to those which apply to ice caves, in that they receive little if any of the direct

rays of the sun, and are also scarcely exposed to any hot winds. It is almost a self-evident proposition that the ice in such gullies is formed in the same manner as the ice of glaciers, or the ice on ponds and rivers, by the cold of winter and the melting of the snows; and when ice is found remaining under boulders in such localities, it is simply that the snow water has run to the lowest level and there congealed. For convenience these gullies may be divided into two classes:

(1) The ordinary open ravines, of which a fine example occurs in the Shawangunk Mountains in New York State.

(2) The ice under boulders, such as we find in King's Ravine.

Ice Sheets.—A subterranean ice sheet covered with lava is reported from Mount Etna, and probably similar sheets also occur in Iceland. Sir Charles Lyell accounts for the one on Etna by the explanation that there must have been a great snow bank in existence at the time of an eruption of the volcano, and the lava stream must have flowed over the snow, which, in course of time, turned to solid ice underneath.

Wind-Holes.—In many parts of the world we find what are known as cold-current caves, or blowing holes, or wind-holes. These are pipes or holes, and in general they are found in more or less broken limestone rocks. In most cases a cool air pours forth from them in summer, while the cold air pours into them in winter, the draught being then reversed. In some cases, however, the operation is said to take place in the opposite way. De Saussure made an experiment in connection with the first kind of these holes, showing what is probably the cause of the cooling of the air current. He passed a current of air through a glass tube, an inch in diameter, filled with moistened stones, and found that the air current which entered with a temperature of 22·5° C., came out with a temperature of 18·75° C., that is, with a loss of 3·75° of heat. As in limestone caverns there would generally be moisture on some of the rock surfaces on which air currents would pass, it is doubtless the evaporation from these surfaces which lowers the temperature of the air cur-

rents. These wind-holes do not seem to necessitate the presence of ice. I have examined five of them myself, and in no case was there any ice visible. Whether there was any ice within the mountain I cannot, of course, say. At Seelisberg, on the Lake of Lucerne, the peasants have built little stone houses over the rocks whence these currents issue, and use these houses for keeping their provisions during the heats of summer. In all the wind-holes I have seen the cool air was pouring out.

Ice Caves.—Ice caves differ greatly from ice gullies. The greatest divergence is that there is a roof. This means that the ice is formed directly in the cave itself, and is not, except perhaps near the entrance, solidified snow, but genuine subterranean ice. The roof, while not admitting the winter snow, is, however, a protection against warm summer rains, and, of course, entirely cuts off radiation from the sky. If, therefore, it keeps out some cold, it also acts as a protector against heat. It may be noticed here that there is a strong resemblance between natural ice caves and artificial ice houses, a resemblance implied in their French appellation, *glacière*, and, in fact, it is hardly too much to look on ice caves as natural ice houses.

Kinds of Ice Caves.—Ice caves proper vary greatly in their positions, shapes and sizes. They may be divided into three main kinds:

(1) Those at or near the base of cliffs, entering directly into the mountain with a down slope. This class is found in limestone and in volcanic rocks. Examples: The Kolowratshöhle, Dobsina, Roth in the Eifel.

(2) Those at or near the base of cliffs, where a long passageway exists before the ice cave proper is reached. All I know of in this class are in limestone rock. Examples: Demenyfálva, the Frauenmauer.

(3) Those where a large pit opens into the ground, and the ice cave is found at the bottom opening into the pit. These are in limestone. Examples: Chaux-les-Passavant and la Genollière.

Ice Caves with Wind-Holes.—In two or three cases there are ice caves in connection with wind-holes; that is, there

is first a large ice cave, at the rear of which is a fissure from which a draught comes. One of these is near Cluses in Savoy, and Professor Thury states that along the line of the draught he found the ice in August more melted than elsewhere in the cave. Another of these ice caves with a windhole is in Washington Territory, and is the largest ice cave so far reported in the United States.

Mines and Freezing Wells.—Subterranean ice is also found in certain places in connection with man's handiwork. It is reported as forming in some few mines in Europe and America. In some wells, also, in the New England States, abnormally low temperatures have been observed, and for four or five months of the winter the surface of the water is frozen so thick as to render these wells useless.

Geographical Distribution.—Ice caves proper are found in various parts of Europe, Asia and America, mostly in the smaller ranges or in the outliers of the snowy ranges, generally in limestone and occasionally in basaltic formations. There are a good many in the Jura, a few in Switzerland, a few in the Italian Alps, a number in the Eastern Alps, in Tyrol, Steiermark and Carinthia. There are some in Hungary, several in Russia, one in Iceland, one on the Peak of Teneriffe, several in Siberia, one in Kondooz in Central Asia, one in Japan, and one in Korea. I have heard, so far, of twenty-nine places where subterranean ice is reported as occurring in North America, two of which are in Pennsylvania. Professors Schwalbe and Fugger give lists which mention over 200 places where subterranean ice is said to occur. However, these lists are not claimed by the authors themselves to be accurate.

Dimensions.—The dimensions of ice caves vary greatly. Some are very large, some very small. The measures of the cave at Dobsina, in the Carpathians, are given as follows: Height of roof above ice floor, 10 to 11 meters; length, 120 meters; breadth, 35 to 60 meters; and surface about 4,644 square meters. The Frauenmauer Cave, in Eastern Tyrol, has an ice floor about 50 meters long by about 7 wide. The ice cave near Frain, on the contrary, is so small that one can only crawl in some 2 or 3 meters, and this at the expense

of soiling one's clothing. In fact, ice caves vary in size between great halls and small tunnels, in which one cannot stand up straight.

Size of Entrances.—The entrances of ice caves vary also greatly as to their dimensions. As a rule they are small, that is, about the size of ordinary door-ways; but the entrance to the Schafloch, for instance, is about 10 meters wide by 7 meters high, while the entrance to Roth in the Eifel is not over 1 meter each way.

Drainage.—An important point in all ice cavès is the need of some outlet for the surplus water at the lowest point of the cave, as otherwise the cave would soon be entirely filled. As the caves are always in either porous or broken rocks, the necessary drainage takes place through the cracks and fissures in the rocks.

Forms of Ice.—Almost all the forms assumed by underground ice are different from those visible in glacier or iceberg. This is only what should be expected, as the ice is formed under such different conditions from those which obtain overground. There are no séracs or crevasses such as one sees *en route* to the Grand Mulets, for instance. The sharp angles and fractures visible on glaciers are absent. Almost all the lines are rounded. Unique forms of subterranean ice are those produced by the drip from roof or sides, that is, the forms of ice answering to the stalactites and stalagmites of limestone caves. They descend from the roof as icicles or rise from the floor as cones or pyramids. Sometimes the icicles reach to the floor, and, connecting with it, become regular columns; or sometimes columns issue from fissures in the sides in the shape of frozen waterfalls.

At Dobsina the stalactites form grand ice pillars. They are from 8 to 11 meters in height, and from 2 to 3 meters in diameter. In some of the caves, as at Chaux-les-Passavant, the ice stalagmites take nearly the form of cones. There are some seven or eight of them, the tallest of which is at least 6 meters high, with a diameter at the bottom of 5 or 6 meters. Sometimes these cones are hollow, as is the case in a grand one at the Schafloch, some 6 meters or more in height.

The columns issuing from the sides of ice caves, for lack of a better name, I call fissure columns. They seem always to issue from a crack in the rock, and then sometimes stream over the rock. Sometimes they spring out far enough from the rock to be quite away from it. These fissure columns vary from about 2 to 7 meters in height, and at the base almost always spread out fan-shaped. Schwalbe speaks of these columns as "subterranean glaciers." Some are found in almost every ice cave; in one case—the Glacière de Chapuis—they form the special beauty of the cave, as there are some seven or eight of them, each perhaps 5 or 6 meters high, and about 1 meter in diameter, and spreading into fan-shape at their base.

The ice on the bottom of the caves of course takes its shape from the shape and angles of the floor of the caves. If the floor is level or nearly so, we find the ice in the shape of a sheet or floor. If the floor of the cave be sloping the ice will be found on more or less of a slope, sometimes becoming nearly or quite vertical, as an ice slope or ice wall, and distantly resembling the portions of glaciers called an ice fall, with the great difference, however, that there are no crevasses, not even tiny ones.* Holes we sometimes find, as at Haut d'Aviernoz, or runnels, as at the Kolowratshöhle, but these are entirely distinct from crevasses. On these ice walls or ice slopes, there is, in fact, no visible sign of anything that would lead to a suspicion of motion.

The holes, or runnels, just referred to are generally found at the lowest point of the ice floors and are almost certainly cut out by the melting water, to which they act as an exit. In fact, they form the drainage system in the ice. The holes are sometimes very deep. At Haut d'Aviernoz I looked down into one, which must have been many meters deep, and whose sides disappeared in darkness, no bottom being visible.

The drip produces also the exact opposite of cones, in the shape of ice basins. There are two splendid ones in the

* No observer seems as yet to have looked for any evidences of motion in subterranean ice.

Kolowratshöhle, each cut out in the floor by an extra strong drip from the roof at those spots. Such basins as these are not to be seen on glaciers.

Lakes and pools are to be found in ice caves. At Chaux-les-Passavant a pool of water, perhaps 30 centimeters in depth and 4 or 5 meters in diameter, lay at one place in August on the ice floor. The whole cave was damp and the ice in places decidedly slushy, in fact, all the signs pointed to a heavy thaw going on, and there could be no doubt that the pool was the result of this. In the Glacière de Chapuis, far underground, there are two adjoining caverns, one filled with water, the other with ice.

Again, in at least one case, the Kolowratshöhle, a lake sometimes forms on the ice floor in the spring, freezes over, and then runs off, leaving the marks of its passage in the shape of ice slabs reposing on the solid ice floor, as was the case on my visit to this cave in the middle of July, 1895.

I think there is no doubt that subterranean snow sometimes occurs, probably early in the year. A case of the kind occurring in Central France has been noticed in Monsieur Martel's book, "Les Abimes," and my brother and I, in a visit to the Dobsina cave early in July, before much thaw had set in, found in one spot of the cave a small sheet, perhaps 2 meters each way, of what, to look and touch, was genuine snow. I have little doubt that this was formed by the congealing, during their fall, of drops of water from the roof.

The ice in caves is sometimes found in a form or structure which is, I believe, of rare occurrence above ground. This is when it occurs in the shape known as prismatic ice. If you break off a lump from a column or icicle you will sometimes notice that it breaks into regular prisms. I have seen this prismatic ice a few times only. Browne speaks of it in his book as common. I am not sure that the phenomenon is as yet satisfactorily accounted for; the only thing I feel sure of is that it does not occur in ice of recent formation. Thury speaks of it as a form which is due to some change in ice which has already been formed for some time.

Ice Slope, Kolowratshöhle.

THE ENTRANCE OF THE KOLOWRATSHÖHLE.

ICE SLOPE, KOLOWRATSHÖHLE.

Color Effects.—The color effect of every ice cavern has a certain individuality. At the Kolowratshöhle the ice is beautifully transparent and of a pale ochre-greenish hue; the limestone rocks are streaked with iron, and thus have a reddish hue, while, owing to the entrance admitting plenty of daylight, the effect is only semi-subterranean. At the Sçhafloch it is the exact opposite of this; the ice is so completely away from daylight that black is the predominating note, the ice itself looking a dark gray. Again, at Chaux-les-Passavant plenty of daylight is admitted; the rocks are a yellowish-brown, while the ice is white and blue. At Dobsina, on the contrary, thanks to the electric light, white is the predominating note, in certain places and corners dulled into grayness by rock or shadow.

II. CAUSE OF THE FORMATION OF ICE.

The cause of the formation of the ice is practically the most intricate thing to understand and explain about ice caves. Numerous explanations have been offered on the subject, and many theories formulated, some of which are absolutely untenable. The great cause, as far as I can see, is simply the cold of winter, which re-forms anew each year the ice which has been destroyed by the heat of the preceding summer. Before elaborating the winter's-cold theory, I wish to mention two or three theories which cannot possibly be accepted in connection with the facts.

Glacial Period.—A suggestion to account for the ice, which seems to occur to many persons when they first hear of ice caves, is that the ice is a remnant of the glacial period. This theory is quite untenable. The great cave of Chaux-les-Passavant was entirely cleared of its ice in 1727 by the Duc de Lévi for the use of the Army of the Saone. In 1743 the ice was formed again. At Szilize, every year, the ice has almost completely disappeared by November, and the cave is free; but in April or May the floor is again covered with ice, and columns and icicles have formed on the roof and sides. At La Genollière, the cave is used by the people of the neighboring chalets through the spring and early summer to help in the operation of butter-making; by the mid-

dle or the end of August it has entirely disappeared, but is found formed afresh every spring. Too many examples of the complete melting away of the ice every year can be cited to permit any doubt on this point; ice caves are not connected with a glacial period.

Salts.—Another favorite suggestion, which cannot stand on an acquaintance with the facts, is that there must be salts in the rocks. An elaborate paper, giving this as the true cause of the ice in Chaux-les-Passavant, was published in 1712, by M. de Billerez, in the proceedings of the French Academy. He stated that the earth in the immediate neighborhood, and especially above the roof of the grotto, was full of nitrous or ammoniacal salts, and he suggested that this salt was disturbed by the heat of summer, and mingled with the water which penetrated by fissures into the cave. His observations have been absolutely disproved, and in no place have any such salts been found. Repeated experiments in letting lumps of cave ice melt in my mouth have convinced me personally that in all cases the water is exceedingly pure and sweet, and there can be no doubt that all, what might be called, chemical causes, must be entirely eliminated as possible cold-producers. The very first notice extant about ice caves, in the shape of a letter dated 1586, of Bénigne Poissenot, a Paris lawyer, about Chaux-les-Passavant, speaks of the deliciousness of the ice water.

Draughts.—The formation of subterranean ice is sometimes—in my opinion incorrectly—assigned to air currents or draughts. Cold caves fall into two divisions in regard to the movements of air—caves with and caves without draughts. The first are ice caves proper; the second wind-holes or cold-current caves. Professor Thury seems to have been the first to call attention to this fact, and he divided caves into static and dynamic caves. The wind-current caves have complicated the understanding of the whole problem, as some observers have assumed that the draughts were the cause of the ice. I think the facts all tend to disprove any such idea, as very few of the observed wind-holes contain any ice. In fact, it seems as if draughts tend entirely to melt, not to form, ice. The air in an ice cave is gener-

ally almost still. I do not remember ever feeling any movement of air in an ice cave proper. In the double cave of Chapuis one cavern is filled with ice and the air is quiet, while the other is filled by a little lake, over which there is a draught. The lake is, doubtless, the result of the draught, which probably melts the ice in summer, if, indeed, it does not prevent any from forming in winter. At the entrances of ice caves I have several times noticed faint movements of the cold air outwards, but this is never perceptible within the body of caves. One cannot exactly feel the air moving out, but by lighting a cigar the smoke may be seen slowly moving outwards. At the entrance of the Kolowratshöhle there was a faint outward current when I was there. The day was a hot one and quite windless, and as the cold cave air met the hot outside air it formed a faint cloud or mist just at the mouth of the cavern.

Evaporation and Atmosphere.—Closely connected with draughts are the evaporation or expansion of the air in caves, and by some persons these are assumed as the cause of the ice. From what I have observed myself, I should say that the dryness or moisture of the air is coincident with the state of freezing or thawing of the cave. If you penetrate into a large cave about the beginning of June, when everything is frozen up tight, you will find no drips nor mushy ice; the air will be relatively dry, and the sensation of cold not unpleasant. If you penetrate into that same cave about the end of August you will find drips coming from the roof, the ice soft, and the sensation of the air a penetrating, damp cold. Thury says that in the caves near Geneva he found in summer the air so saturated with moisture that it could scarcely take up any more. In his observations with a psychrometer in the Grand Cave de Montarquis in August, he found $\frac{92}{100}$ of relative moisture. Although from what I at present know, I am inclined to disbelieve in draughts, evaporation, or expansion causing ice to form, yet I think there is need for a great deal more observation on this point.

Capillary Theory.—Another theory which has found some acceptance is the so-called capillary theory; namely, that

bubbles of air, drawn into water, flowing down through fissures in the rock, are liable to a continually increasing pressure, compelling it to part with latent caloric, which is immediately absorbed from the water on being liberated in any cave or well or mine. There may be truth in this, and I have been assured that in some borings in Western mines ice has been formed by pressure; but if it applied to caves, I do not see why so many thousands of caves contain no ice, and especially why ice caves are never known in hot countries. If the theory were correct, we should, for instance, find ice in the caves of Yucatan, described by Mr. Mercer. Still, this theory is held by some scientific observers. The Hungarian doctor at Dobsina told me he believed in it, and Dr. Schwalbe, whose opinion is entitled to great respect on account of his many observations, thinks that the rocks in summer are a cooling factor, and, that some cold comes out from them.*

Summer Ice Formation Theory.—The theories just mentioned are advanced by well-educated persons, not necessarily scientists. The natives and peasants in the neighborhood of ice caves generally believe that the ice of caves is formed in summer and melts in winter. This belief I have met with everywhere—in the Eifel, Jura, Swiss Alps, Tyrolese Alps and Carpathians. Peasants and guides will tell you with absolute confidence: "The hotter the summer, the more ice there is." The hotel keeper at Eisenerz, near the Frauenmauer Cave, told me "many people had broken their heads" in trying to account for that cave freezing harder in August than in May. This belief is founded principally on the fact that the natives scarcely ever go to an ice cave except when some tourist takes them with him, and therefore they rarely see one in winter, and their opinion is, consequently, of no value. Thury tells a good story about this belief of the peasants. He visited the Grand Cave de Montarquis in midwinter. All the peasants told him there

* The capillary theory was first advanced by Mr. N. M. Lowe, of Boston, and published in the *Science Observer* for April, 1879. An admirable exposition of the theory by Mr. John Ritchie, Jr., was also published in the same number of the paper.

would be no use going, as there would be no ice in the cave. He tried to find even one peasant who had been to the cave in winter, but could not. He then visited it himself, and found it full of hard ice. On his return he told the peasants of his discovery. They were staggered at first. Finally one exclaimed: "It makes no difference; in genuine ice caves there is no ice in winter."

Still, there is an appearance of truth in the peasants' belief, and that is in the fact that the temperatures of ice caves, like that of other caves or that of cellars, are colder in summer than the outside air, and are warmer in winter than the outside air. Possessing neither reasoning powers nor thermometers, the peasants simply go a step further and say that the caves are cold in summer and hot in winter. In other words, from sheer ignorance, out of a true fact they make nonsense.

The extraordinary thing is that any number of writers—sometimes scientific men—in books and scientific papers, have accepted this belief of the peasants and tried to account for it. An attempt at explaining it dates back as far as 1686, in the Paris *Histoire de l'Academie Royale des Sciences.* It is found in a German book printed in 1689, then again in a French scientific journal in 1712. In the *London Philosophical Transactions*, in 1739, there is a long account in Latin about the cave of Szilize, in which the writer, a Hungarian *savant*, Mathias Bel, describes how, as soon as the warmth of spring comes, the cave begins to freeze, and, as the weather gets hotter, freezes the harder, until, in the dog days, icicles and columns grow with immense rapidity. But when the winter comes the ice all melts away, the cave becomes warm and pleasant, and the insects and the bats, and then the hares and foxes of the neighborhood come there in droves and live amicably together during the cold months. Several dozen writers in our century accept and repeat these statements as the truth. Sometimes the statements are repeated with, sometimes without, the hares and foxes, the latest repetitions occurring in 1861, 1876, and 1881.

Winter's Cold.—I now turn to the explanation which seems to me the true one, both because it is the simple and

obvious one, and because all the facts, as far as I have personally observed, entirely tally with it. This is the theory that the cold air of winter is the chief factor in producing the ice, and that it is through its means that certain caves are converted into what are practically natural ice houses. That the cold of winter produces the ice is hinted at by Gollut in 1592, by De Boz in 1726, and by Cossigny in 1743; it is elaborated into something like a theory by Prévost in 1789, and independently suggested by Townson in 1793; it is mentioned by Humboldt in 1814 and Deluc in 1822. Worked at afresh and elaborated into a theory by Thury in 1861 and Browne in 1865, it has received confirmation in our own time by the work of Professor Fugger and Captain Trouillet.

All my own observations have tended more and more to make me believe that the cold of winter is the cause of the ice, and to make my thought clearer I will put it in the form of two propositions. The first is:

The ice in caves is formed entirely by the cold of winter; the heat of summer tends to melt it. Owing to the sheltered position of cave ice, however, the summer heat reaches it with more difficulty than it reaches the snow and ice in the open, and cave ice, therefore, remains sometimes long after the ice in the surrounding country has disappeared.

The second is:

Two things are necessary for the formation of ice. The first is cold, the second is water. Therefore, to form ice in a cave, the cold air of winter must have access to it, and in some way water must be supplied to it.

Altitude and Latitudes.—The most important proof, to my mind, of the truth of these propositions, is the fact that no ice cave that I have heard of, is found in any latitude or below any altitude where ice and snow does not form for part of the year in the surrounding open country. None are reported from India or Africa, or, in fact, from any low-lying places in tropical latitudes. They are found mostly in middle latitudes, and only where during part of the year, at least, there is a cold season, that is, where, for some time, the thermometer stands below freezing point. Even in the middle latitudes they are in general at fairly high altitudes.

The Schafloch is at 1,780 meters; Skerizora in Transylvania at 1,127 meters; Dobsina at 1,100 meters; the Glacière de Saint George at 1,208 meters. It is true, there is one cave in the sub-tropical latitude of Teneriffe, La Cueva de la Nieve; but that is at an altitude of 3,300 meters, and where snow falls every year in the open on the Peak. Unless some cave is hereafter discovered in a region where there is no winter, I do not see how the imperative necessity of the presence of the cold air of winter for forming the supply of ice can be controverted.

Temperatures.—That the cold air of winter is the important factor in the production of cold seems to me also proved by the thermometric observations recorded in various caves by different observers. They all tell the same tale, that the temperatures vary with those of the outside air; that they are lowest in winter and highest in summer. Here are the results of one year's observation at Dobsina in degrees centigrade:

January,	− 4·2	July,	+ 2·1
February,	− 3·4	August,	+ 3·8
March,	− 2·1	September,	+ 2·3
April,	− 1·2	October,	+ 0·2
May,	+ 0·9	November,	− 1·9
June,	+ 1·5	December,	− 3·2

Here are a year's observations at Frain:

January,	− 5	July,	+ 3 to + 5
February,	− 5 to − 2	August,	+ 5
March,	− 1 to 0	September,	+ 3 to + 6
April,	0	October,	+ 5
May,	+ 2 to + 5	November,	+ 5
June,	+ 3 to + 6	December,	0 to − 2

These figures seem to me to prove pretty conclusively that from about the first of November to the first of May we have—inside the ice caves—winter temperatures, that is, temperatures below freezing point; and from about the first of May to the first of November we have summer temperatures, that is, temperatures above freezing point.

Positions of Entrance and Body of Caves.—A great and important factor in permitting the cold air to permeate, and remain in, a cave, is the actual form and position of the cave

and of the entrance. In all known cases, as far as I can learn, the main body of an ice cave is well below the level of the entrance, and even if the ice cave is sheltered against the wind, it is not sheltered against the cold air of winter. This is heavy, and by its own weight sinks well down to the bottom, freezing up, in course of time, all the moisture that may drip from the roof or that may come into the cave in the shape of melted snow or cold winter rain. The summer air, being warm and, therefore, light, can only enter the cave much more slowly to dislodge the winter air and destroy the ice, and before it accomplishes the latter, another winter's freeze reverses once more the conditions. This applies universally to the main body of the caves.

In all but two known cases there is a steady drop from the entrance of an ice cave to the ice. These exceptions are the Posselthöhle and the Frauenmauerhöhle, in which you first ascend gradually a short distance before the drop to the main ice begins. Both these caves are at high altitudes and well sheltered. In the Frauenmauer you have to walk through a gallery for about 30 meters, ascending in that distance perhaps 3 meters, when the slope is suddenly reversed and you then have a down grade. At the highest point, last July, I found quite a large mass of ice, enough to fill, perhaps, a couple of ice carts. But as, in ascending to the cave, I had crossed the remains of two snow avalanches, it does not seem very remarkable that better sheltered subterranean ice should remain, even if higher than the entrance of the cave. I, therefore, should not feel surprised at any time to hear of an ice cave being found with the body of the cave higher than the entrance; but this would be either at a very high altitude or in a very high latitude.*

The position of the entrance is very important. In almost all cases it has a northerly exposure, and is sheltered against

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* Since this paper has gone through the press, I have heard of a cave, "Amarnath," in the Himalayas, where the floor is said to slant upwards to the back wall. Two blocks of ice, which last through the year within the cave, are worshipped by the Hindus under the names of Siva and Ganesh, probably as re-incarnations.

entering winds. If these two conditions do not exist the ice supply surely suffers. Sometimes the entrance is more or less tortuous. In some cases it is protected by a fringe of trees. Again, in one kind of ice cave there is first a great pit, at the bottom and on one side of which we find the ice cave proper. In one case at least, the Geldloch on the Ötscher, the entrance is due south, and as a consequence the ice in the front of the cave, solid about the end of June, becomes a lake about August, and then cuts off access to the rear of the cave. In the case of the Kolowratshöhle the entrance is badly sheltered against the wind, and this undoubtedly affects the supply in summer, and causes more rapid melting there than in some other cases.

Time of Formation of Ice.—As already mentioned, cold and water are the necessary requisites for the formation of ice, and the time and method of formation were approximately explained by Thury. His winter excursions caused him to accept as proved that part of the mountaineers' belief which holds that there is no ice formed in caves in winter. He says: "In winter the cold is not wanting, but if there is no spring emptying into the cave, water is absent, and then no ice is formed. It is in the spring, at the time of the first melting of the snows, that the ice must be formed. Then water at 0° C. flows on the surface, and penetrates by the fissures of the rock and by the great openings into the interior of the chilled cavern, which receives besides the freezing night air. The grotto then makes its yearly provision of ice, which could do nothing more than diminish during the entire duration of the hot season."

Dr. Terlanday recently published a paper about the Cave of Szilize, in which he asserts that ice does not form there in winter, but that the ice first forms in the winter in the upper part of rock fissures, and that in the spring, at the time of an increase of temperature, this fissure ice is brought to the melting point by the successive entering of heat into the earth, and this water then arrives at the cave, where it aids the formation of icicles. This theory about fissure ice is probably in so far correct, that the ice in the upper parts of fissures, near the surface of the ground, melts before the

ice in the lower parts of fissures. The drip would then naturally run into the cave, and as long as the temperature of the cave was below freezing point would help to form cave ice.

But this theory does not take sufficiently into account surface thaws. Thury's theory is very good and true enough in many respects, but it does not go far enough. The observations of Professor Fugger, and especially those of Captain Trouillet, make certain the fact that ice begins to form in a cave as soon as the temperature of the cave has sunk below freezing point, whenever, from any cause, water gets into the cave. This will occur whenever there is a thaw outside at any time during the course of the winter, and it is probably entirely correct to assert that ice is formed intermittently in caves all through the winter months.

In conclusion, I wish to say that I look on this paper as in the nature of a *preliminary report* only. I have collected a great deal more information about ice caves, which I hope to publish before long. The whole subject is as yet by no means exhausted, and further observations may bring forth valuable results, and I wish I might now see American scientists give more attention to the ice caves of our own continent.

RECENT ADVANCES IN THE STUDY OF THE RESINS.*

By Henry Trimble.

HISTORY.

The earliest knowledge of the resins was probably derived from frankincense and myrrh; it is possible, by certain paintings on the walls of the ancient temple of Dayr el Báhri, in Upper Egypt, to recognize that they were important articles of traffic seventeen centuries before Christ; they were associated with such important commodities as

* Abstract of a lecture delivered before the Franklin Institute, December 4, 1896.

gold, ivory and silver. Amber, likewise, was held in great esteem by the ancients, and its history can be traced back to the twelfth century B. C.

The Mosaic writings of the Bible make frequent reference to *incense*, the chief ingredient of which was frankincense or olibanum. Later, a number of other resins appear in history; mastic is mentioned as existing in the fourth century B. C.; Venice turpentine also became known about that time, and a number of the other secretion appeared then or earlier. Galbanum, mastic and several other resins were mentioned three centuries B. C. Dioscorides recorded a still larger number in the first century of the Christian era.

The use of such resins as frankincense, myrrh, galbanum and mastic, in the preparation of their incense by the ancient Israelites, went far towards establishing the value of these drugs and in stimulating the trade in them. Some of these resins are used as incense at the present time.

The electrical properties of the resins early attracted the attention of the human race, and it is merely necessary to recall the fact that the word electricity took its name from amber because of that property. Amber was, and still is, consumed in large quantities by the Mohammedans in their worship.

Another resin, which is of interest on account of its antiquity, is scammony. Theophrastus, in the third century B. C., was acquainted with it, and it has been held in high esteem ever since.

Asafœtida was also in high repute by the ancients; it was used as a condiment in the tenth century, and the more fetid the drug the better it was liked.

These few illustrations may serve the purpose of establishing the antiquity of the resins, and especially the fact that they were considered of the greatest value by the people of ancient days.

DISTRIBUTION OF THE RESINS.

The resins are so widely distributed throughout the vegetable kingdom that there is, perhaps, no plant but will

yield some resinous substance, either by secretion or by extraction with alcohol. All parts of the plant, too, yield one or more of these substances, with the exception of the cambium, which appears to be free from resinous constituent. As a rule, the bark contains resin most abundantly, although in a few cases, as in certain conifers like the larch and our Southern pines, the sap wood contains more, and appears to be capable of secreting it almost indefinitely.

The conifers of the present time, as well as those of a former epoch, have been the great producers of resins. Those in tropical and semi-tropical climates have been found to yield them most abundantly. Usually resins are associated in plants with volatile oils, and such exudations are frequently known as balsams.

A number of resins occur in nature, which are the product of trees and plants not existing at the present time; for example, amber is supposed to have originated from pines of the Miocene period. Copal is also found under conditions which indicate that it is not derived from the vegetation of to-day, with the exception, of course, of certain of the cheaper varieties. Much of the true copal gives evidence of having been under water for a long period. In the study of the resins, therefore, it will be seen that we are confronted with some of the most serious problems in geology, and, as our knowledge of the chemistry of these substances becomes more exact, we may be able to assist in answering some geological questions by their aid.

THE PHYSIOLOGY OF THE RESINS.

This subject is closely allied to the occurrence of the resins. It is a subject, however, on which at present we have very little light.

Botanists have investigated it with the aid of the microscope, and have, consequently, drawn conclusions at variance with those of chemists.

A number of the former have concluded that resins result from the destruction of cell walls, and are, therefore, the decomposition product of cellulose.

Wiesner* believed that the starch of the cell contents contributed to the formation of the resins, and that this was effected by the aid of a middle group, namely, the tannins.

Chemists, on the other hand, usually maintain that the resins originate from the terpenes, or from aldehydes.

It is difficult to accept the theory of their origin from starch when we consider that very little starch exists in some of the tissues which yield resin most abundantly, as in the long-leaf pine. That tannins have a close relation to the formation of resin is generally conceded. The following argument in support of this has been advanced by E. S. Bastin:† "It is pertinent to note in this connection that all the facts observed regarding the oleoresins of the pines show that they are very closely associated with the tannins. While this of itself does not prove that the former are derived from the latter, the nature of the association is such as to strongly suggest such a conclusion. For example, a secretion reservoir begins in a cluster of a few thin-walled cells, rich in granular protoplasm, which early shows an abundance of tannin. Later on, oleoresinous matters appear, and, as these increase in quantity, the tannin and the protoplasm diminish, and, finally, the walls break down, leaving a cavity or intercellular space containing the oleoresin. In the meantime, cells immediately bounding this space are gradually undergoing similar changes, and so on, as long as the secretion reservoir continues to grow. So, if any well-developed secretion reservoir with the surrounding cells be examined, there will be found:

"(1) A central space filled with oleoresin.

"(2) An area of cells immediately surrounding this, which contain much oleoresin, and a little tannin and protoplasm.

"(3) Still farther exterior, a layer of very granular cells, rich in protoplasm and tannin, but containing very little volatile oil or resin.

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* *Sitzungsbericht, der k. Akad, zu Wien.*, Bd. 51; also "*Die Rohstoffe des Pflanzenreiches*," p. 69.

† *Am. Jour. Pharm.*, **68**, 137.

"There is no denying the fact that, as the resin increases, the tannin diminishes, whatever the conclusion we may draw from the circumstance.

"The view that tannins are derived from starch apparently obtains no support from these observations on the pines.

"In regard to the origin of resin from volatile oil, the microscopic study of the pines, especially of Pinus palustris, seems to afford pretty clear evidence in the affirmative. Old secretion reservoirs have been observed to contain irregular solid or semi-solid masses of oleoresin, in which, apparently, the resin is the predominating element, while young reservoirs contain a more fluid oleoresin in the form of globules. Moreover, in the secretion cells immediately surrounding the reservoirs, the oleoresin is in globules and evidently very fluid. In fact, in passing from the younger to the older portions of the secretion tissue, there appears to be every gradation between a very liquid volatile oil and a semi-solid resin.

"There appears to be no question that the oleoresin is to be regarded wholly as a waste product. It clearly can play no part in the process of nutrition. Its only apparent use is that of protection against the destructive forms of animal life and against vegetable parasites. It is highly antiseptic, it protects mechanically against injurious insects, and its taste and effects are disagreeable to most of the higher animals."

That the resinous secretion may, in certain conifers, be diverted, so as to be an entirely different substance, closely resembling the sugars in physical properties, is seen in the sweet secretion of the sugar pine, Pinus Lambertiana, of California. While this substance does not belong to the carbohydrates in a strictly chemical sense, still, it is as closely allied to them as is phenol to the alcohols of the paraffin series.

Why one pine should secrete sugar and its near neighbor, botanically and geographically, should secrete resin is a question wide open for speculation, as well as for exact botanical and chemical research. In a number of cases gums of the carbohydrate series are a part of the resinous secretion whereby we get the gum resins.

GENERAL PROPERTIES OF THE RESINS.

The resins, as a class, vary too much from one another to have many properties in common. There are, however, a few characters which have already been cited in defining them, and which belong to all of them. They are all insoluble in water, mostly amorphous, rarely opaque, being either transparent or translucent. When pure, they are odorless and tasteless, but as they are associated, in a number of instances, with volatile oils and aromatic acids, they often have an aromatic odor and a bitterish taste. They are mostly easily melted at 100° to 150° C., although the harder copals melt above 200° C. They are usually soluble in alcohol and turpentine, and many of them are dissolved by benzol, petroleum ether, carbon disulphide and the fixed oils. The resins neither decompose nor volatilize, and they may remain unchanged in the earth or under water for centuries. An illustration of their keeping qualities occurred in 1885, when a lot of benzoin was offered for sale in London that had been raised from a wreck off the southwest coast of Africa, which occurred in 1691. The resin was found to be of excellent quality, yielding 92 per cent. of its weight to alcohol, and containing more than the ordinary amount of benzoic acid. Presumably, it should have lost some of the latter constituent during its 200 years' sojourn at the bottom of the ocean.

CHEMICAL COMPOSITION.

With the chemistry of the resins is closely allied their classification. Their classification must eventually be a chemical one, if they retain their identity as a class. Liebig was satisfied with a separation into hard resins and soft resins. This classification had previously been adopted by Berzelius, although the latter also spoke of a classification which distinguished those resins which exuded from trees spontaneously, and those which were extracted by alcehol.

About the same time, Unverdorben classified the resins according to their behavior towards ammonia. This investigator's classification is sometimes referred to by writers of the present day, but the only value it has now is historical.

He did establish a number of the properties possessed by the resins, especially their behavior towards alkalies and some metallic salts; also the electrical properties of a number of them. It may be remarked here, however, that in "Lavoisier's Chemistry" (1806) is a short account of the behavior of resins towards alkalies, and the statement is briefly made that "with alkalies it forms soap, and is much used in manufacture for that purpose, being a chief ingredient in yellow soap, and that to which it owes its solidity."

While the edition from which this quotation was taken was issued some time after the author's death, still the property of resin to form soap was, no doubt, known much earlier.

Hirschon,* in 1878, published a "scheme for the recognition of the more important resins, gum resins and balsams. While this is of considerable value analytically, it does not add to our knowledge of the chemical composition of these bodies.

We find in the text-books of to-day a nomenclature of the resins which stands in the way of a clear comprehension of their composition and properties. For instance, benzoin is said to consist of α, β, γ and δ resins, a conclusion of over half a century ago, based on the action of certain solvents. The analysis on which such a statement is based was published in *Liebig's Annalen* in 1840, and looks something like the following:

	Per Cent.
Benzoic acid	3·00 to 14·5
α resin	3·0 to 3·5
β resin	52·0 to 48·0
γ resin	25·0 to 28·0
δ resin	0·8 to 0·5
Foreign matter	5·2 to 5·5

Hlasiwetz,† in 1867, contributed a paper on the relations of the tannins, glucosides, phlobaphenes and resins with one another, and both he and Rochleder added considerably

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* *Pharm. Zeit. für Russland*, **16**, 81, and *Am. Jour. Pharm.*, **50**, 130.

† Ueber die Beziehungen der Gerbsäuren, Glucoside, Phlobaphene und Harze. Von H. Hlasiwetz, *Sitzungsberichte der math.-naturw. Classe* **55**, (II), p. 575.

to our knowledge of the various resins, although no comprehensive system of classification was accomplished.

Flückiger also added much to the knowledge of individual resins, especially in reference to their history and sources.

This brings us down to the present time; and during the summer just passed Professor Tschirch, of Berne, Switzerland, has published a classification based on a study of individual resins, by himself and his students, which investigation has been progressing for a number of years. It is to be hoped that this work will be continued, for the results on the individual members of the class thus far investigated point in the direction of simplicity.

Copal, dammar, sandarac, dragon's blood, asafœtida, galbanum, ammoniac, sagapenum, opoponax, acaroid, benzoin, tolu, balsam peru, the fruit of liquid amber, storax, amber and gutta-percha have been investigated, and the following bodies have been found as the chief constituents:

(1) Resin esters, or their derivatives.

(2) Resin acids (resinolacids).

(3) Resenes, indifferent bodies of unknown origin.

Only very few of the resins contain members of all three groups. Much oftener they consist of only one member of the group.

The odor, when present, is due to ethereal oil or aldehyde, or, oftener, to a very small quantity of an ester of cinnamic acid, especially the phenyl-propyl ester of that acid.

I. The resin esters, or those containing aromatic acids are divided into two classes. One consists of compounds with benzoic acid, and the other with cinnamic acid.

(1) Benzoic ester is contained in balsam peru, balsam tolu, siam benzoin and dragon's blood. Ammoniac contains an ester of salicylic acid. Benzoyl acetic acid is contained in dragon's blood.

(2) Cinnamic ester is contained in balsam peru, balsam tolu, storax, sumatra benzoin, yellow acaroid.

An ester of cumaric acid is contained in yellow and red acaroid-resin.

An ester of ferulic acid is contained in asafœtida.

Umbelliferic acid and its anhydride, umbelliferon, exist in combination in asafœtida, galbanum and sagapenum.

Most of the aromatic acids forming esters in resins are oxy-acids.

The only acid of the fatty series forming a resinous ester is succinic acid in amber.

The resin esters contain resin alcohols, which are either colorless and do not give the tannin reaction, or are colored and give the tannin reaction. The former are called *resinoles*, the latter *resinotannoles*.

(*a*) Resinoles.—Of these, four are known:

Succinoresinol,	$C_{12}H_{20}O$, in amber.
Storesinol,	$C_{12}H_{20}O$, or $C_{36}H_{56}O_3$, in storax.
Benzoresinol,	$C_{16}H_{25}(OH)O$, in benzoin.
Thirinol,	$C_{28}H_{47}OH$, in opoponax.

Spectrum analysis of their solutions in concentrated sulphuric acid shows storesinol and benzoresinol to be closely related. Succinoresinol and storesinol have the same percentage composition. All the members of the resinol group are related among themselves, and all belong to the aromatic series.

(*b*) Resinotannoles.—The following are known:

Siaresinotannol,	$C_{12}H_{13}O_2 \cdot OH$, in siambenzoin.
Sumaresinotannol,	$C_{48}H_{19}O_3 \cdot OH$, in sumatrabenzoin.
Peruresinotannol,	$C_{18}H_{19}O_4 \cdot OH$, in peru balsam.
Toluresinotannol,	$C_{17}H_{17}O_4 \cdot OH$, in tolu balsam.
Galbaresinotannol,	$C_{18}H_{29}O_2 \cdot OH$, in galbanum.
Ammoresinotannol,	$C_{18}H_{29}O_2 \cdot OH$, in ammoniacum.
Sagaresinotannol,	$C_{24}H_{27}O_4 \cdot OH$, in sagapenum.
Dracoresinotannol,	$C_8H_9O \cdot OH$, in dragon's blood.
Ponaxresinotannol,	$C_{34}H_{49}O_7 \cdot OH$, in opoponax.
Xanthoresinotannol,	$C_{42}H_{66}O_{10}$, in yellow acaroid.
Erythroresinotannol,	$C_{40}H_{40}O_{10}$, in red acaroid.

Six of these show a multiple of six carbon atoms.

In many other ways there is a close relationship among the various resinotannoles.

It is evident that the resinotannoles contain but one hydroxyl group in the molecule.

The ready formation of picric acid by treatment of the resinotannoles with nitric acid leads us to believe that the

hydroxyl is attached to the benzol nucleus, and not to a side chain. On fusing with alkali hydrate fatty acids are formed; in some cases, protocatechuic acid or resorcin is produced. The resinotannoles belong to the aromatic series.

II. The resin acids exist in the resins in the free state; they are, so far as investigated, oxy-acids, that is, contain hydroxyl and carboxyl. The following have been separated and investigated:

Podocarpinic acid,	$C_{17}H_{22}O_3$, in podocarpus resin.
Abietinic acid,	$C_{44}H_{64}O_5$, in colophony.
Pimaric acid,	$C_{20}H_{30}O_2$, in pine resin.
Succinoabietinic acid,	$C_{80}H_{120}O_5$, in amber.
Sandaracolic acid,	$C_{45}H_{66}O_7$, in sandarac.
Callitrolic acid,	$C_{65}H_{84}O_8$, in sandarac.
Trachylolic acid,	$C_{56}H_{86}O_2$, in copal.
Isotrachylolic acid,	$C_{56}H_{86}O_2$, in copal.
Dammarolic acid,	$C_{56}H_{90}O_8$, in dammar.
Guaiacic acid,	$C_{20}H_{26}O_4$, in guaiac.
Guaiaconic acid,	$C_{19}H_{20}O_5$, in guaiac.
Copaivic acid,	$C_{20}H_{30}O_2$, in copaiva.

Many of these resins are found to be related to one another.

III. The most difficult class of resins is, without doubt, the *resenes*. Their resistance to reagents has given them the synonym of indifferent material, and has made them difficult to classify. They are neither hydrocarbons, alcohols, esters, acids, ketones or aldehydes, but belong, so far as investigated, to the aromatic series.

The following have been examined:

α ponaxresen,	$C_{32}H_{54}O_4$, in opoponax.
β ponaxresen,	$C_{32}H_{52}O_5$, in opoponax.
α dammarresen,	$C_{32}H_{52}O_3$, in dammar.
β dammarresen,	$C_{31}H_{52}O$, in dammar.
Fluavil,	$C_{40}H_{64}O_4$, in gutta-percha.
Alban,	$C_{40}H_{64}O_2$, in gutta-percha.
α copalresen,	$C_{25}H_{38}O_4$, in copal.
Dracoalban,	$C_{20}H_{40}O_4$, in dragon's blood.
Dracoresen,	$C_{26}H_{44}O_2$, in dragon's blood.
Myroxoresen,	$C_7H_{10}O$, in myroxylon fruit.

When the formulas of these are carefully studied they are found to have some relation among themselves.

If it be asked what is the practical value of a classifica-

tion like this? the answer may be found in the present knowledge of benzoin compared with what it was a few years ago, when this resin was said to consist of benzoic acid and alpha, beta, gamma and delta resins. At the present time we know that this resin is a compound of benzoic acid with benzoresenol, and a compound of resinotannol, the latter varying somewhat in the different varieties.

Further than stating that the resinotannoles are peculiar compounds or mixtures of tannins and resins, we cannot at present go, except that they contain one hydroxyl group and appear to have a multiple of six carbon atoms. Certainly the chances of discovering the secret of the chemical constitution of these bodies by this road are very materially advanced, even if the task does appear somewhat formidable.

GRAPHICAL DETERMINATION OF THE INDEX OF THE POWER ACCORDING TO WHICH ONE QUANTITY VARIES RELATIVE TO ANOTHER.

By Prof. W. F. Durand.

It often happens in engineering investigations that a curve is found expressing the simultaneous relationship between two quantities; for example, an indicator diagram showing the relation in the cylinder between pressure and volume; or a diagram showing the relation between engine economy and point of cut-off or load; or between the resistance or power of a ship and her speed; or between the head of water-pressure on an orifice and the velocity or amount of efflux; or between the work required to remove a lathe chip and the depth or speed of cut, etc.

Now, with all such curves it is frequently desirable to determine the instantaneous index of the power according to which one of these quantities varies relative to the other. We may thus say that at a certain point in the stroke of a steam engine, the pressure, according to the diagram, varies inversely as the 1·1 power of the volume; that is, at that

point, the curve is similar to one passing through the same point, and having the equation $p\,v^{1\cdot1} = \text{const.}$

Similarly it may be found that at a certain speed the resistance of a ship varies as the 2·3 power of speed, or at a certain load the total water consumption of an engine as the square of the load, and similarly for other relationships.

This form of expression is quite common, and the index serves a real purpose in indicating the relation between the ratios of increase of the two quantities, and therefore how much more or less rapidly one is increasing relative to the other.

In order to gain a more distinct idea of the mathematical nature of this index, let us take, as an illustrative example, the relation between the horse-power and the speed of a given ship, and let us fix our attention first on the horse-power required for a given speed, say ten knots. Then let a slight increment be given to the speed, such that the new value divided by the old is equal to $(1 + e)$, where e is a small fraction. As a result, the required horse-power will be increased by an increment such that the new value divided by the old will equal $(1 + f)$, where f is also a small fraction, but not the same as e. Then we may evidently put:

$$(1 + f) = (1 + e)^m$$

whence

$$m = \frac{log.\,(1 + f)}{log.\,(1 + e)} \qquad (1)$$

This furnishes a definition of m in accordance with our natural ideas as illustrated in the opening paragraphs. It follows that if the speed is increased in the ratio $(1 + e) \div 1$ the power must, as a result, be increased in the ratio $1 + e)^m \div 1$; so that if $m = 3{\cdot}5$, and the speed is increased, 1 per cent., the power must be increased in the ratio $(1{\cdot}01)^{3\cdot5} \div 1$, or sensibly 3·5 per cent.

It is readily seen that thus defined, the value of m would depend on the particular value of the increment e. In order then to have a definite mathematical basis for the definition of this index, it is taken as the limit of the value to which the above definition leads when the increment e is indefinitely decreased.

As another view of the nature of m, let us expand $(1 + e)^m$, remembering that since e is very small, all powers beyond the first are negligible in comparison with it. We have then:

$$(1 + f) = (1 + e)^m = (1 + m\,e)$$

whence

$$m = \frac{f}{e} \qquad (2)$$

That is, if the speed is increased by a very small part of itself, measured by the fraction e, then the power must be increased by a very small part of itself, measured by the fraction $m\,e$. This relation is exact when e is indefinitely small, and for most engineering purposes will be found sufficiently so when e is not greater than about 1 per cent.

We thus have two definitions of m which will lead to identical results when e is very small as defined. It may be mentioned in passing, however, that if m is constant (a case rarely met with in empirical relationships), then the value as given by (1) will be independent of the value of e. In such case the curve could be represented by an equation of the form $y = ax^m$, in which m is a constant. In general, such curves must be represented by an equation of the form $y = ax^n$ where n is variable, and not equal to m as here defined. Reference may be had elsewhere* for a more detailed examination of the relation of these two exponents.

The nature of the exponent m being now clearly before us, we will proceed to give methods for its determination.

In *Fig. 3*, let y_1 and y_2 denote the ordinates of the curve at A and B, and denote $O\,C$ and $O\,D$ by x_1 and x_2. Then assuming that m is sensibly constant between A and B we have, as an analytical value:

$$m = \frac{log.\ y_2 - log.\ y_1}{log.\ x_2 - log.\ x_1} \qquad (3)$$

Where many such exponents are to be found, this method is quite tedious, and it will be found much quicker and more satisfactory to make use of the following simple geo-

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* *Journal Am. Soc. Nav. Eng'rs*, 5, 543.

metrical determination, to draw attention to which is the chief purpose of this article.

At the given point P draw a tangent PR and note the foot of the ordinate Q. Then

$$m = \frac{OQ}{RQ} = \frac{\text{abscissa}}{\text{sub-tangent}} \qquad (4)$$

The proof for these two values of m will be found in the appendix, for those who may be interested.

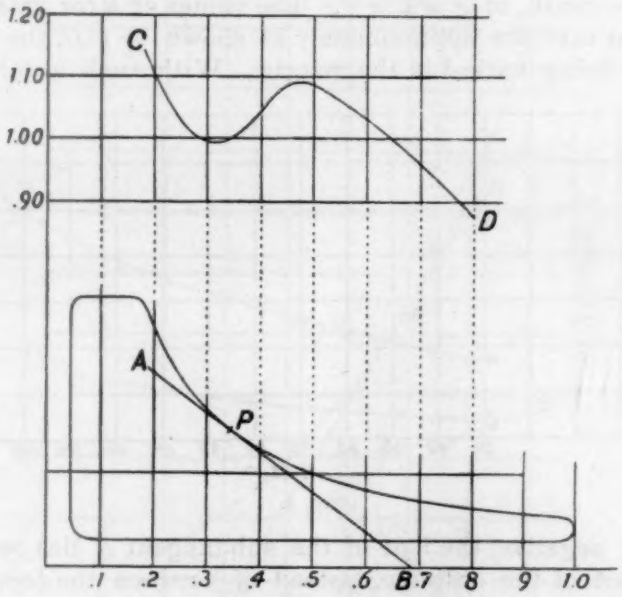

FIG. 1.

The drawing of a tangent to a given curve is subject to some geometrical uncertainty. Practically, it will be found possible to determine the direction of the line with all necessary accuracy. The tangent may be drawn simply by judgment, or somewhat more accurately by the following construction:

Assuming that the curve in the immediate neighborhood of P, *Fig. 3*, may be represented by a second degree parabola, we take two points, A and B, such that the abscissa of P is a mean between those of A and B. Or, in other words, we

take two points, C and D, at equal distances from Q, and taking the corresponding points on the curve, we have A and B. Then a line through P, parallel to the chord $A\,B$, will give the tangent desired. The proof is readily deduced from the well-known properties of second degree parabolic curves.

We will now give a few examples:

In *Fig. 1* we have an indicator card from a Corliss pumping engine, in which the equation to the expansion line is $p\,v^n = \text{const.}$, or $p = C\,v^{-n}$. The values of n for this particular card are approximately as shown by $C\,D$, the ordinates being marked in the margin. With such a curve, n

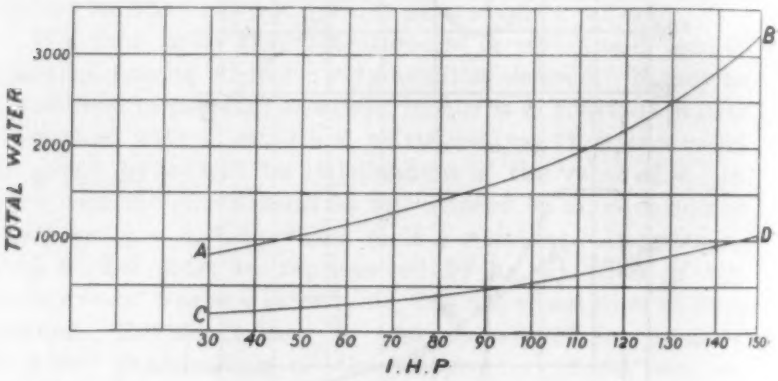

FIG. 2.

being negative, the foot of the sub-tangent B lies beyond the foot of the ordinate, instead of between the foot and the origin, as in *Fig. 3*. In any case, however, the value of m is the quotient of the abscissa divided by the sub-tangent.

The question of the causes of variation in the index, as shown in this diagram, is beyond the purpose of the present article, in which we are merely concerned with the determination of its value.

In *Fig. 2* let $A\,B$ give the total water consumption for a certain engine developing I.H.P. as given by the abscissæ. Then, each vertical division being unity, $C\,D$ shows the value of the index with which the water consumption

varies relative to the power developed. At 95 I.H.P. the total water varies directly as the power, and this is the point of minimum consumption per unit I.H.P., and, hence, the point about which there is the least variation in efficiency for slight changes in power.

At 130 I.H.P. the total water varies as the 1·7 power of the I.H.P., while at 150 I.H.P. the index has risen to 2·1, so that the variation is a little greater than as the square.

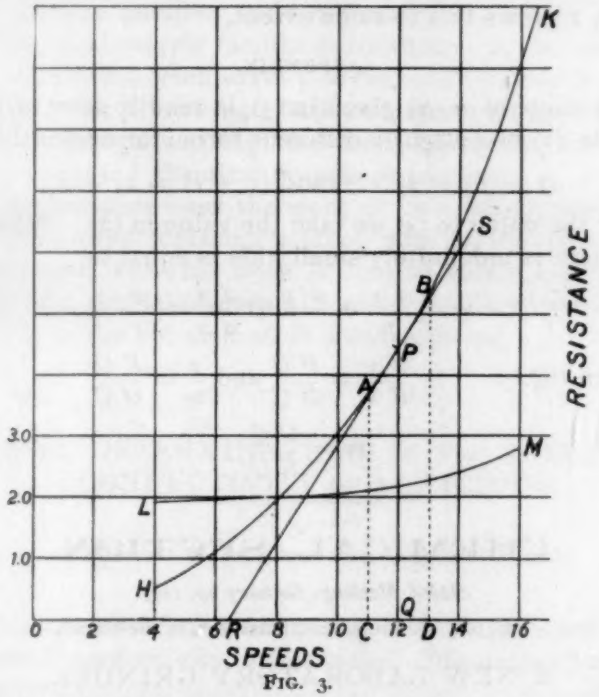

FIG. 3.

For values of I.H.P. below 95 the index decreases, showing that the rate of decrease in total water is less rapid than in power. The cause of these variations is, of course, the decreasing efficiency on either side of 95 I.H.P.

In *Fig. 3*, *H K* represents the curve of resistance of a ship at varying speeds. Then *L M* gives the variation of the index for the same conditions. At 9 *k* the resistance varies as the square of the speed, falling off slightly at lower speeds and increasing quite rapidly beyond 12 *k*.

Many other illustrations might be given, but these will serve to show the application of the method.

It will be found that the determination of this index furnishes a very delicate test of the continuity or discontinuity of algebraic law, which the curve, as drawn, represents. Many curves, which to the eye may seem smooth and continuous, will be found to have a more or less irregularly variable value of the index m. The indicator curve of *Fig. 1* shows this to some extent.

APPENDIX.

The value of m, as given in (3), is readily seen to be the value in (1) in a slightly different form; for evidently

$$(1 + f) = y_2 \div y_1 \text{ and } (1 + e) = x_2 \div x_1.$$

For the value in (4) we take the value in (2). When the increment is indefinitely small, this is equal to

$$m = \frac{d\,y}{y} \div \frac{d\,x}{x} \text{ or } \frac{d\,y}{d\,x} \div \frac{y}{x}$$

Now, in *Fig. 3*

$$\frac{d\,y}{d\,x} = \frac{P\,Q}{R\,Q} \text{ and } \frac{y}{x} = \frac{P\,Q}{O\,Q}$$

Hence

$$m = \frac{O\,Q}{R\,Q}$$

CHEMICAL SECTION.

Stated Meeting, January 19, 1897.

DR. JOS. W. RICHARDS, President, in the chair.

A NEW LABORATORY GRINDER.

NOTES BY CHAS. A. BUCK, A.C.,
Chief Chemist, Bethlehem Iron Company.

This grinder was designed by Maunsel White, mechanical engineer of the Bethlehem Iron Company, and the first one made has now been in constant use in the laboratory of that company for three years. In that time it has been worked an estimated average of five hours per day, and, so far, no part has broken, nothing worn out, nothing gotten out of repair.

It has been used there principally for iron ores (hematites and refractory magnetites), limestones, fuels, refractory materials (fire-clays, magnesite, sands, etc.).

A general average of its speed of working is that it will pulverize a charge of 30 grams of refractory magnetite from 80 mesh to an impalpable powder in fifteen minutes. By hand, this would require about one hour's steady work, and even at the best the powder would not then be so uniform. The greater fineness and uniformity of powder over the hand-ground sample facilitates solution in acids, and, in the case of fusions, often saves a re-fusion of the undecomposed residue. On an average, half an hour to an hour's time is saved in the pulverizing, and half an hour to two hours in the subsequent chemical process of solution.

The machine does the work of two strong boys in half the time, thus replacing a labor item of probably $300 to $500 a year, while the work is done so much more satisfactorily that this laboratory will, under no circumstances, ever go back to the old method of hand-grinding.

RECENT DETERMINATIONS OF THE ELECTRICAL CONDUCTIVITY OF ALUMINIUM.

By Joseph W. Richards and John A. Thomson.

Many and various values have been determined for the electrical conductivity of this metal. The causes have been as follows:

(1) The impurity of the metal used. Until 1886, the best commercial aluminium rarely surpassed 98 per cent. in purity, and it was not until 1889 that commercial metal of 99 per cent. was put on the market. As will be shown later, the effect, even within these narrow limits, is to change the conductivity nearly 10 per cent.

(2) The reference of the conductivity to copper or silver as standards. In such cases, the exact purity of the copper or silver and the physical condition of these metals, whether

hard or soft, must be known in order to give the comparison its proper value; but these were in most cases either unknown or neglected. Even at the present time, the absolute conductivity of pure soft copper or silver cannot be said to be fixed closer than within 1 per cent., so that figures for conductivity of aluminium, given only with reference to copper or silver, cannot, at best, have an accurate significance.

(3) Lack of an accurate standard of absolute resistance. The adoption of standard units of resistance, by international concert, and the consequent multiplication of registered copies, has made it an easy matter to use in experiments certified instruments of accurately-known resistance, and thus to dispense with self-constructed units of comparison in favor of more accurate standards.

(4) Imperfect methods of measurement. Of late years, several ingenious arrangements have been devised for eliminating from the calculations of experiments the resistance of connections, always an uncertain quantity, and more refined instruments for measuring and balancing electric currents have been constructed, thus permitting of increased accuracy in results.

In the following experiments, the specimens tested were kindly furnished by the Pittsburgh Reduction Company, and were all analyzed by Mr. Handy, of the Pittsburgh Testing Laboratory, so that their composition was accurately known. The conductivity is given in absolute measurement, so that no reference to any other metal as a standard can affect the results. This was rendered possible by the use of a certified standard resistance coil of 1 "International" ohm, whose possible error is not over 0·02 per cent., and by the use of the *Carey Foster* method of comparison. The metal was in wire, of 50-foot lengths, the diameter of which was measured by a micrometer and checked by weighing and determining the specific gravity. The wires were wound on wooden bobbins and immersed in oil, the temperature of which was given by a thermometer. The galvanometer used was a reflecting instrument, sufficiently delicate for all purposes. The standard coil was

immersed in water, and the room was kept at a constant temperature. The bridge wire used was carefully calibrated, and all readings were taken several times. Two separate wires were tested in case of specimen 1, the result given being the mean of two results, which differed only one-hundredth of 1 per cent. from each other.

	Analysis.						Resistance at 0° C. of a Wire, 1 Meter Long by 1 Millimeter Diameter, in Ohms.	Specific Resistance at 0° C., *i. e.*, Resistance of 1 Cubic Centimeter in Absolute (C G.S.) Units of Resistance.	
	Aluminium.	Iron.	Copper.	Silicon.	Sodium.	Zinc.	Hard.	Hard.	Annealed.
(1) .	99·66	0·10	0·00	0·16	0·008	—	0·031245	2453·7	2432·2
(2) .	99·58	0·25	0·00	0·16	0·052	—	0·03290	2584·0	2535·0
(3) .	98·77	0·20	0·57	0·45	0·012	—	0·03627	2848·0	—
(4)	97·16	0·25	2·26	0·30	0·032	—	0·03590	2819·6	—
(5) .	94·39	0·25	3·07	0·24	0·052	1·50	0·03583	3011·4	2984·7

For the reduction from the working temperature to 0° C. an experiment was made with wire No. (1), which showed that between 27° C. and 0° C. its temperature coefficient was 0·00392 per degree. This coefficient was used for the nearly pure wires, while for (4) and (5) a slightly lower coefficient, determined by Mr. Scott, was used. It appears that the purer the metal the greater its temperature coefficient.

Conductivity tests of a similar set of wires were made by Mr. C. F. Scott, electrician of the Westinghouse Electric Company, Pittsburgh. They were made by comparison with pure copper, with a Wheatstone bridge. These results can only be compared with ours by assuming a certain value for the conductivity of copper, and even then we cannot say how nearly the copper used by Mr. Scott would approach that standard. Sir W. Thompson's value for the specific resistance of copper is 1580, Dewar's 1562. In the following table we reduce our results to each of these standards, and add Scott's results for comparison:

RELATIVE CONDUCTIVITY (COPPER = 100).

	Richards and Thomson. Using for Copper the Resistances (1580)	(1562)	*C. F. Scott. Actual Resistance of Copper Employed not Known.*
(1) Soft	65·0	64·2	—
(1) Hard	64·4	63·7	63·1
(2) Soft	62·3	61·6	—
(2) Hard	61·1	60·5	62·2
(3) Hard	55·5	54·9	56·2
(4) Hard	56·0	55·4	58·5
(5) Soft	52·9	52·3	—
(5) Hard	52·5	51·9	55·0

TEMPERATURE COEFFICIENT FOR 1° C.

	C. F. Scott. (Between 15° and 80° C.)	*Richards and Thomson. (Between 0° and 27° C.)*
(1)	·00385	·00392
(2)	·00385	—
(3)	·00360	—
(4)	·00361	—
(5)	·00359	—

In connection with the results of Mr. Scott and ourselves, we may mention for comparison those of Charpentier-Page, who used what he calls *pure* aluminium, which may safely be assumed to be the No. 1 grade of European aluminium, averaging 99 per cent. pure. He finds as follows:

	Specific Resistance. (Calculated to 0° C.)	*Compared with Copper.* (1580) *Per Cent.*	(1562) *Per Cent.*
Soft	2659	59·4	58·8
Hard	2684	58·9	58·2

It should be noticed that these results fall exactly between our Nos. 2 and 3, also just where its composition would most probably lie. The results also agree closely with ours in showing almost exactly 1 per cent. greater conductivity for the annealed than for the hard-drawn wire.

Dewar and Fleming have also recently found as the specific resistance of "Swiss aluminium about 99 per cent. pure" the value 2563 at 0° C., which is 60·9 per cent. of that of copper, according to their own measurements. This also fits in well with our determinations, but the comparison would have been much more satisfactory if the exact composition of their metal had been determined.

C. K. McGee determined, in 1890, the conductivity of aluminium analyzing 98·52 per cent. pure to be 54·8 per cent. that of copper, when unannealed. This metal was nearly identical with our No. 3 in composition, and the results are the same within 1 per cent.

The conclusions we would draw from these experiments and comparisons are that—

The conductivity of hard-drawn commercial aluminium is strongly affected by impurities, being, approximately:

		(*Copper* = 100)
98·5	per cent. pure aluminium	55·0
99·0	" " " "	59·0
99·5	" " " "	61·0
99·75	" " " "	63·0 – 64·0
100·0	" " " "	probably 66·0 – 67·0

Annealed wire has a conductivity very nearly 1 per cent. greater than the unannealed.

LEHIGH UNIVERSITY, January 19, 1897.

ELECTRICAL SECTION.

Stated Meeting, December 22, 1896.

MR. CLAYTON W. PIKE, President, in the chair.

EQUALIZING CONNECTIONS FOR COMPOUND-WOUND DYNAMOS COUPLED IN MULTIPLE.

BY E. R. KELLER.

The successful operation of stations for electric lighting and power depends largely on the automatic regulation, not only of the speed of the prime movers, but also of the E.M.F. of the generators. In both of these essential features the last few years have brought forth marked development, in consequence of which we are now enabled to install plants for electric lighting and power, in which the speed of the engines will not vary more than 1½ per cent. from the normal, when the full load is suddenly thrown on or off, while the builders of dynamo machinery can and do supply us with generators which are almost absolutely self-regulating as to E.M.F. A modern constant potential generator will not only compensate automatically for the variable losses due to armature reaction and internal resistance, but it can be made to give an E.M.F. at the brushes which, under varying conditions of load, will be such as to compensate not only for the variable losses in the machine itself, but also those due to the resistance of the external circuit.

The success which has thus been attained in the automatic regulation of constant potential generators is largely due to the careful study and application of compound winding; and while but a few years since, nearly all of the large stations were equipped with plain shunt-wound generators, we find that these have now been superseded by the compound-wound type, in almost every class of service.

This change from shunt to compound winding has necessitated a slight modification in the connections from dynamo to switchboard, where two or more generators are coupled in parallel. For such an arrangement of generators we must employ an additional conductor between the generators, commonly called the equalizer or tie line, whose function it is primarily to prevent a possible reversal of polarity of any of the machines, and further to equalize to some extent the output of each machine under varying conditions of the external load or speed of the prime movers.

That this connection is absolutely an essential for the successful operation of compound-wound generators in multiple is beyond dispute. On the other hand, there has been a great deal of dispute in regard to what strength of current it may be called upon to transmit. In fact, its mode of action does not seem to be entirely clear to many engineers who are perfectly familiar with the installation of dynamo machinery, and who would not think of omitting the equalizer where two or more compound machines are to be used in multiple. I desire to submit a mechanical analogy, which, though deficient in many of its details, will serve to show the action of compound machines together with the equalizer connection and the function which it has to perform.

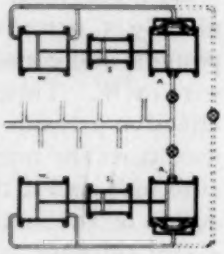

FIG. 1.

Consider, for a moment, the arrangement shown in *Fig. 1*. We have two direct-acting steam pumps located at different points along a system of distributing mains. These pumps consist of a steam cylinder *S*, a water cylinder *W* and an auxiliary cylinder *A*, through which all the water passes before it enters the supply mains. There would be no particular utility for such an auxiliary cylinder in a pump; in fact, unless the details of such an arrangement are carefully worked out, it is more than likely that such a combination would not work at all. It will suffice, however, for the sake of the analogy, and we will assume that the details are so

arranged that the mechanism will operate in the manner desired. Suppose now we have a steam pressure of 100 pounds per square inch in the steam cylinder, a pressure of 70 pounds in the cylinder *W* and 60 pounds at the beginning of the supply mains, under normal conditions. The 10 pounds difference between the 70 pounds and 60 pounds, acting on either side of the auxiliary piston, which is intended to act in a manner similar to an hydraulic engine, will help the steam piston along, and so long as the two machines continue to act under normal conditions, everything will go smoothly, each machine taking its share of the load. But now suppose that the steam pressure in one of the engines should fall to, say, 95 pounds. We would immediately have a corresponding drop of pressure in *W* to, say, 65 pounds, while in the supply mains the pressure would still remain constant. We would now have an effective pressure of only 5 pounds in the auxiliary cylinder instead of 10 pounds, and consequently a still greater reduction of pressure in *W*. This, in turn, would still further diminish the effective pressure in *A*, and so on. Presently the back pressure from the main will be greater than the forward pressure in *A*, and if the difference becomes sufficiently great, the action of the auxiliary cylinder will overpower that of the steam cylinder, and the whole mechanism will be driven backwards by the water pressure in the mains. The machine will not only cease to contribute its share of water to the supply mains, but will actually consume enough to operate it as a motor, and thus make considerably more work for the other machine.

To avoid the possibility of this reversal taking place, we connect the water pipes where they enter the auxiliary cylinders, as shown by the dotted lines. If we make this tie line sufficiently large, there will always be supplied enough pressure to the front of the auxiliary piston to prevent the pressure in the mains from reversing the mechanism, and furthermore, to equalize in a measure, the work done by each unit, under varying conditions—the equalization being the more complete, the larger the tie line.

This is precisely what takes place in compound- or series-

wound generators; and if we consider the cylinders *W* as replaced by armatures, the auxiliary cylinder by series or compound windings, the steam cylinders by the shunt windings, and the steam pressure acting on the piston by the speed of rotation of the armatures, we have two compound-wound generators coupled in parallel.

Fig. 2 shows this arrangement as commonly used. The equalizer connection, as here shown by the dotted lines, was first suggested by Gramme for series machines, and by Mordey for compound. If made sufficiently heavy, it will not only effectually prevent the reversal of polarity of any of the machines so connected, but will, in a great measure, equalize the work done by the generators under varying conditions of speed. Any number of dynamos may be connected up in this manner, and, even if they are of different

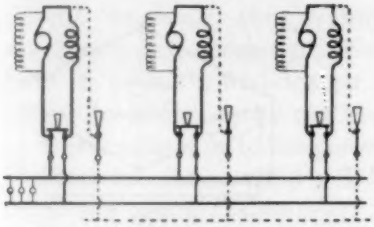

FIG. 2.

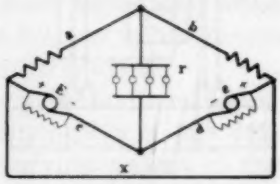

FIG. 3.

capacities, each machine will give current in proportion to its rated output, provided the combined resistances of the leads to the bus bars, the armatures and the series fields, are inversely proportional to the ampère capacities of the machines. Without the equalizer connections, or if these are not sufficiently heavy, a change of speed or of load is liable to produce a reversal of polarity in the deficient machine, in which case it will run as a motor, supplying none of its share of the current, but, on the contrary, making an additional load for the other machines, and perhaps causing a costly interruption in the operation of the plant.

The question now arises—how shall we proportion the equalizer connection?

In *Fig. 3* we have a diagrammatic representation of two compound-wound generators, of equal capacities, in paral-

lel, supplying current to a common line of resistance, r, and having an equalizer connection of resistance, x. So long as the E.M.F.'s generated by the two machines are the same, equal currents will flow through the series fields and there will be no current in the tie line. If, however, the E.M.F. (E) of one machine becomes greater than that (e) of the other, there will immediately be a greater current in the series fields of the machine generating the higher E.M.F., and a flow of current in the tie line tending to equalize the currents in the series fields, which current we will call X. But this flow of current in the equalizer will cause a fall of potential $= Xx$, and so long as there is a fall of potential in this line, the currents in the series windings, which we will call A and B, respectively, can never be equal. Hence, it

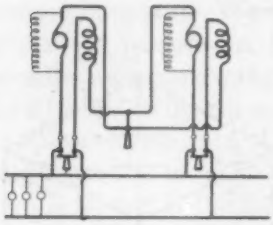

FIG. 4.

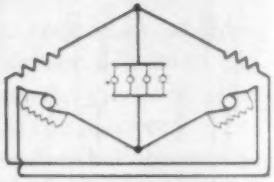

FIG. 5

follows that we can only have A equal to B when $x = o$, which is evidently impossible.

A little consideration will further show that even if we could make $x = o$, we would not have succeeded in equalizing the currents in the armatures, but merely those in the series windings, because, if there is any inequality in the E.M.F.'s, that armature which is generating the higher will carry not only the current in its own series windings, but, in addition, that supplied to the other machine to make the two equal. That is, it will carry a current $C = A + X$, while the other armature carries only $D = B - X$. In other words, if the resistance of the equalizer could be made equal to zero, so that $A = B$, one armature would carry more current than the other by an amount equal to twice that in the equalizer.

It is evident, therefore, that in this method we effectually

prevent the reversal of polarity of any of the machines so connected, while we only tend to equalize the currents in the series windings, the tendency being the more nearly realized, the less the resistance of the tie line.

There is another method of attaining this end, and one which, if it could be successfully applied, would attain it much more perfectly. This method, which is an old one, is illustrated in *Figs. 4* and *5*.

It consists in passing the current of one armature around the series coils of the other machine, and *vice versa*. The method is perfectly feasible, and the mutual action of the output and strength of field of the two machines insures an equal division of the load.

Fig. 4 shows how this might be arranged at the switch-board, and it will be seen that by closing the single pole switch either machine could be operated alone. Unfortunately, however, this system can be used only for two machines, and moreover, these two must be exactly alike both in capacity and design. As this state of affairs rarely occurs in practice, the method has but little value.

Returning now to the consideration of the first-mentioned method, let us see what will be the distribution of the currents for given resistances, and for varying values of the E.M.F.

If, in *Fig. 3*, we let

a = resistance of positive lead and series field;
c = resistance of negative lead and parallel resistance of armature and shunt field;
} for the machine whose E.M.F. $= E$;

b = resistance of positive lead and series field;
d = resistance of negative lead and parallel resistance of armature and shunt field;
} for the machine whose E.M.F. $= e$;

r = resistance of external circuit;
x = resistance of equalizer circuit, and

A, C, B, D, R and X = respectively, the currents in the different branches of the circuit, we have, from Kirchoff's laws:

$$C = A + X \tag{1}$$

$$R = A + B \tag{2}$$

$$D = B - X \tag{3}$$

$$Cc + Xx - Dd = E - e \tag{4}$$

$$Cc + Aa + Rr = E \tag{5}$$

$$Dd + Bb + Rr = e \tag{6}$$

Solving these equations for A, B and X, we obtain, by making the necessary transformations and reductions:

$$(A + X)\,c + Xx - (B - X)\,d = E - e$$
$$(A + X)\,c + Aa + (A + B)\,r = E$$
$$(B - X)\,d + Bb + (A + B)\,r = e$$

$$Ac - Bd + X(c + d + x) = E - e$$
$$A(a + c + r) + Br + Xc = E$$
$$Ar + B(b + d + r) - Xd = e$$

$$A(a + c + r)(c + d + x) + Br(c + d + x) + (E - e)\,c - Ac^2 + Bcd = E(c + d + x)$$

$$Ar(c + d + x) + B(b + d + r)(c + d + x) - (E - e)\,d + Acd - Bd^2 = e(c + d + x)$$

$$A[(a + c + r)(c + d + x) - c^2] + B[r(c + d + x) + cd] = E(d + x) + ec$$

$$A[r(c + d + x) + cd] + B[(b + d + r)(c + d + x) - d^2] = e(c + x) + Ed$$

$$A = \frac{[(b + d + r)(c + d + x) - d^2][(d + x)E + ec] - [r(c + d + x) + cd][e(c + x) + Ed]}{[(a + c + r)(c + d + x) - c^2][(b + d + r)(c + d + x) - d^2] - [r(c + d + x) + cd]^2} \tag{7}$$

$$B = \frac{[e(c + x) + Ed][(a + c + r)(c + d + x) - c^2] - [r(c + d + x) + cd][E(d + x) + ec]}{[(a + c + r)(c + d + x) - c^2][(b + d + r)(c + d + x) - d^2] - [r(c + d + x) + cd]^2} \tag{8}$$

$$X = \frac{E - e - Ac + Bd}{c + d + x} \qquad (9)$$

from which the currents in all the branches of the circuit may be calculated, if the resistances are known and the E.M.F.'s assumed.

It may be of interest to show, from an example, what is liable to occur in actual practice.

At one of the hotels in this city the electric lighting plant consists of two direct connected compound-wound multipolar generators, intended to give 400 ampères each at 115 volts. The internal resistances of the machines are as follows:

	Armature.	*Series Coils.*	*Shunt Coils.*
First machine	·011	·0041	15·5
Second machine	·015	·0036	11·5

The leads of the machines and the equalizers, which are arranged as in *Fig. 2*, are all about 20 feet long (to the switch-board), and they are made of a cable of 350,000 circular mills cross-section, so that

$$a = ·0047 \text{ ohms},$$
$$b = ·0042 \text{ ohms},$$
$$c = ·0116 \text{ ohms},$$
$$d = ·0156 \text{ ohms},$$
$$x = ·0012 \text{ ohms},$$
$$r = ·1437 \text{ ohms (at full load)}.$$

Since the machines are direct connected, there is a possibility of a variation in speed in the two dynamos, and, furthermore, there is quite a little difference in the characteristic curves of the two machines, although the field frames and armatures were intended to be exact copies of one another. Hence, a change of load may produce a material difference in the generated E.M.F.'s. Let us assume that, at full load, both machines were generating exactly 115 volts, at which voltage the current in each armature was 400 ampères, but that a change of load and speed has caused the pressure of one of the machines to fall to say 110 volts. If then we substitute the values given above for the inter-

nal resistances and for E and e, the values 115 and 110, respectively, we have from (7) and (8)

$$A = 390 \text{ ampères},$$
$$B = 374 \text{ ampères},$$

for the values of the currents in the series coils. That is, for a difference of 5 volts in the two machines, we have a difference of only 16 ampères, or about $3\frac{1}{2}$ per cent. in the series field currents.

The current in the equalizer from (9) is

$$X = 222 \text{ ampères},$$

and in the armatures from (1) and (3)

$$C = 612 \text{ ampères},$$
$$D = 152 \text{ ampères}.$$

In other words, a difference of 5 volts in the E.M.F.'s of the two machines has caused a flow of current in one armature which is over four times as great as that in the other, while the equalizer has made the currents in the series windings nearly equal, there being a difference of only $3\frac{1}{2}$ per cent.

Of course, these results are not accurate, because we have entirely neglected the resistances of the switches, fuses and contacts in the calculation. At the same time the results show to what extent the generators might be unbalanced under adverse conditions; and, moreover, it is evident that the equalizer may be called upon to carry currents of very considerable strength.

There are three factors which exert an unbalancing influence on a system of generators coupled in multiple:

(1) A variation of speed in one or more of the generators.

(2) A variation in the external load.

(3) A variation in the strength of the magnetizing current.

The first of these, speed variation, is, perhaps, the most serious, and we cannot attach too much importance to the matter of inherent regulation of the prime movers, the belt transmission and other factors which affect the speed of the generators.

The second, variation of the external load, affects the distribution of the load the more, the greater the divergence between the characteristic curves of the machines. As already stated, the development in this respect during the past few years has been very decided, and there is, as a rule, but little trouble from this source.

The third unbalancing factor, viz.: a change in the strength of the magnetizing current, is of a secondary nature, and would, evidently, not exist were the speed of the prime movers and the E.M.F. of the generators absolutely self-regulating. This is not the case, however, and, consequently, any variation in the distribution of the load due to the first two causes is aggravated by the third. As already stated, it is the function of the equalizer to remedy this, and it was shown that this cannot be done absolutely except by making the resistance of the equalizer = 0.

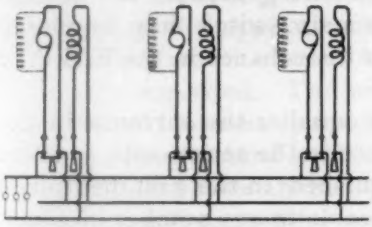

FIG. 6.

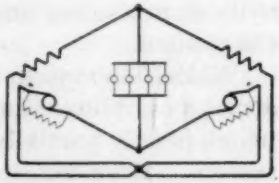

FIG. 7.

In the arrangement of Gramme and Mordey the equalizer must always have an appreciable resistance, and consequently there will always be a loss of potential in it. Moreover, this loss of potential is always at the expense of the weaker machines.

I desire, in conclusion, to suggest an equalizing arrangement in which the drop of potential in the equalizer is divided equally among the machines, in consequence of which the currents in the series windings of all the machines are equal under all conditions. The arrangement is shown in *Figs. 6* and *7*.

It will be seen that this is accomplished by connecting the beginnings of all of the series coils to an extra bus bar, to which is connected also one brush of each machine, in-

stead of connecting the series windings direct to the brushes and the junctions of these to the equalizing bus bar, as in Gramme's method.

By arranging the connections in this manner it is evident that the currents in the series coils of the machines will, at all times, be proportional to the capacities of the machines, provided the resistances are proportioned in such a way that the drop of potential from the brushes of all the machines to the bus bars will be the same at full load.

Fig. 6 shows how this method would be arranged at the switchboard. The mode of operation would be as follows: Whenever it is desired to throw in an additional machine the single pole switch is first closed. This completes the series field circuit, and we immediately have a current in the series coils of the new machine, which will be proportional to its capacity. The rheostat in the shunt circuit is then adjusted until the new machine generates the desired E.M.F. After this is done the main switch may be closed without producing any further disturbance in the E.M.F. of the system.

While this system does not equalize the currents in the armatures, it does equalize those in the series coils, an effect which is only partially accomplished in the first-mentioned system. Moreover, it is applicable to any number of generators of equal or of different capacities, and in no way interferes with the continuity of operation, while the throwing in or out of additional units is greatly facilitated.

It has these disadvantages, however: It necessitates an additional conductor from the dynamo to the switchboard, and, moreover, the full current passes through the conductors which here replace the equalizer. Hence, the first cost is somewhat increased, and, further, a certain amount of energy is lost in heating the conductors. In very large installations, where the distance between dynamos and switchboard is considerable and where the currents are of great magnitude, the loss of energy which this method involves may be sufficient to preclude its application. In smaller installations, however, the increase in first cost and the loss of energy in the additional conductors will be inappreciable.

ELECTRICAL SECTION.

Stated Meeting, December 22, 1897.

MR. CLAYTON W. PIKE, President, in the chair.

X-RAYS, APPARATUS AND METHODS.

BY ELMER G. WILLYOUNG AND H. LYMAN SAYEN.

That Professor Roentgen's discovery of the X-ray has initiated many lines of thought, promising to greatly extend our knowledge of physical phenomena as well as to revise many of our previously accepted views, is generally admitted. That the practical results accruing to humanity by virtue of the applications of this discovery in surgical and medical practice, are of even greater promise and value, is equally conceded. The writers have been engaged for a number of months past in developing apparatus and methods for practical work with especial reference to the needs of the physician and surgeon. For conciseness, we have arranged the matter under consideration in a series of subordinate heads, each of which we shall briefly discuss. Your programme committee has thought that some of the results secured might be interesting, in view of the fact that very little literature regarding the technique of X-ray work has thus far been published.

The Coil or Generating Source.—Thus far the only apparatus known which will produce X-rays readily and profusely is the "transformer." By this, however, we do not mean the commercial transformer of every-day use, but its earlier, and for most purposes less efficient form, the "induction coil." Such a transformer gives exceedingly great electromotive forces capable of producing discharge over long air gaps. When the discharge from such a coil is passed through properly exhausted and constructed tubes, we have a very vigorous generation of X-rays.

Two arrangements of the induction coil are advocated. The one is known as the Tesla or "high-frequency" coil, and the other is the direct and old-fashioned use of the simple induction coil, in which the high secondary E.M.F. is delivered direct to the terminals of the tube.

The High-Frequency Coil.—Fig. 1 shows the diagram of the Tesla combination or "high-frequency" coil. It consists, as will be seen, of two induction coils, the induced secondary current of the first being used to charge a Leyden jar. The primary of the second coil is joined, in series with a spark gap, to the two coatings of the Leyden jar. When the Leyden jars are fully charged, they discharge across

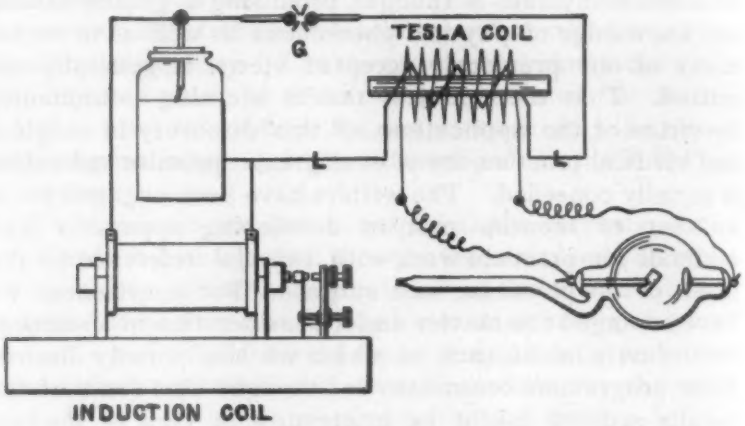

FIG. 1.

the air-gap, this discharge being, by well-known laws of electrical circuit flow, an oscillating one of exceedingly high frequency—millions or more oscillations per second. We thus have the primary of the second coil excited by an alternating current of exceedingly great voltage and frequency, so that *its* secondary produces a discharge of still *greater* E.M.F. and of this same high frequency. So great is this final E.M.F., that it is generally necessary to immerse the entire second coil, primary and all, in a tank of oil, since no solid insulation has yet been found capable of standing these great E.M.F.'s without breakdown.

To excite the Tesla coil, we may use either an interrupted direct current or an ordinary alternating circuit joined directly to the primary of the first coil. But the final result is, in either case, an alternating, high-potential, high-frequency current. In the tube, therefore, we also have an alternating discharge.

The Induction Coil.—We illustrate, in *Fig. 2*, the scheme of connections, etc., of the simple induction coil. The primary must be excited by an interrupted direct current, which may be secured from primary batteries, storage batteries, or a commercial circuit as may be determined by convenience or inclination. The secondary is joined directly to the tube

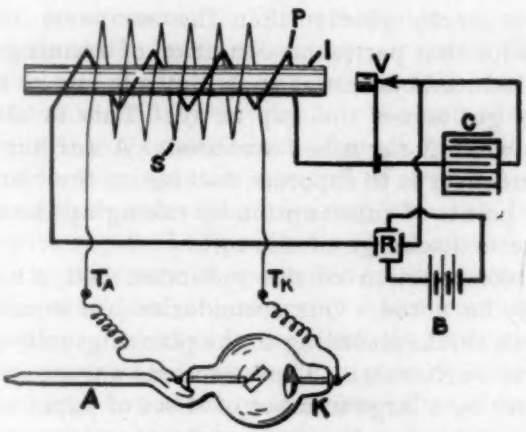

FIG. 2.

terminals. The usual means employed for securing the interruptions is some form of vibrating spring device, automatically operated by the core of the coil itself, the magnetic pull of the core attracting a mass of iron which, by its motion, separates two contact points, thus allowing a properly placed spring to draw it back, closing the circuit again, when the cycle is repeated. Such a device is employed in most forms of virbating electric bells and is indicated in the diagram. Around the break is placed a condenser. At break, this condenser charges (thus preventing the extra induced current due to the large self-induction of

the primary), but, being "short-circuited" by the primary, at once discharges about the iron core in a direction *opposite* to the regular "make" current, thus reducing (if correctly proportioned) the magnetic intensity of the iron core to zero. All this takes place so quickly that the practical effect of the action is merely to assist and intensify the dropping off of the core's magnetism—to shove it along—hasten it. Considering the secondary, now, we find that we shall have at *break* an induced discharge of greatly higher E.M.F. than that at *make*, owing to the falling off of the core's magnetism *at* break being greatly sharper than its growth at make. The effect of this is to give secondary discharges in *one direction only* at all times when the spark gap is not *greatly* shorter than the *maximum* obtainable spark gap for that particular condition of running the coil, the *make* induced current at such times being of too small E.M.F. to get across the gap at all. This is always the condition when X-ray tubes are used. A further function of the condenser is to suppress *burning* at the "break" terminals or points of interruption by taking up the energy of the magnetic discharge of the coil.

In the construction of the induction coil, a number of points may be noted. Our secondaries are wound in sections $\frac{1}{8}$-inch thick (according to the plan originally proposed, we believe, by Ritchie). These sections are separated from one another by a large number of discs of paper of a brand especially selected as free from carbon particles, and baked at a temperature a little below charring point for some time immediately before use—this to drive out moisture. These sections are then assembled and immediately immersed in a special insulation composition having a very high melting point, with high specific heat, and some slight viscosity at all temperatures.

Paraffine we absolutely prohibit as being apt to crack and absorb moisture. The melting point of paraffine, also, is so low (not over about 140° F. for the highest), that in warm weather there is serious risk of displacement of some of the sections by their own weight; for the same reason but little energy can be dissipated from the secondary.

Another grave danger in the use of paraffine is that due to the acid which it usually contains. This attacks the wire, forming copper sulphate, which dissolves gradually into and throughout the mass of the paraffine, thus effectually destroying its insulating properties.

After cooking for some time in the insulation, to drive out the last traces of moisture, the stack of sections is subjected to a further treatment, by which it is finally cooled *with all air removed.* This we consider a point of the highest importance, as air present anywhere in the secondary becomes electrified and bombards to and fro, gradually softening the insulation and eventually breaking down the coil. (For confirmation of this point, see Tesla, on "High-Frequency Phenomena.") With this plan of construction we have no static leaks of energy, or small direct leaks within the coil itself, and deliver the full energy of discharge at the secondary terminals. We find ourselves able to secure in this way a full inch of spark in all sizes of coils with considerably less than 1 pound of wire, No. 34 B. & S.* We believe that, more than anything else, the large quantities of wire required to produce a given spark length with many of the coils now in use is due to the presence of air, and consequent loss of energy by static bombardment, in the secondary. As regards the insulating composition used by us, we may say that we find its power of resistance to spark discharge to be four or five times that of hard rubber. In the arrangement of our secondary, we separate it from the primary by a heavy hard-rubber tube, and, in addition, by a tube of this composition.

The Adjustable Condenser.—A coil is working to best advantage when there is a certain definite relation between its primary current, secondary spark distance, and condenser capacity. Frequency of break must also be considered. As coils have heretofore been built, however, the condenser value has been fixed once for all and admits of no change, albeit both spark gap and primary current may be so

* In our latest coils not over three-quarters of a pound to the inch of spark is used.

changed. To remedy this, we have devised a form of switch shown in the diagram, *Fig. 3*, by means of which the condenser capacity may be shifted, at will and instantly, by simply turning the switch. The effect upon the secondary discharge is very marked, both volume and musical note of the spark changing with the position of the switch. We find this idea exceedingly convenient in general experimental work with alternating currents, it being possible with it to alter condenser capacity as quickly and as readily as we may self-induction or resistance. (We have made some experiments in the use of a condenser in parallel *with the primary*. The volume of the secondary discharge seems, in many cases, to be greatly increased.)

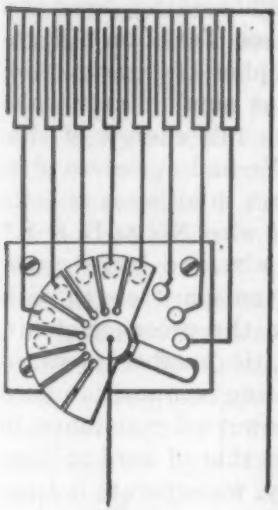

FIG. 3.

In arranging coils to operate from few or many cells of battery it is, of course, merely a matter of winding the primary with coarse or fine wire, the number of ampère turns being kept the same. It is interesting to note, as we have noted experimentally many times, that the larger the primary E.M.F. the smaller must be the condenser capacity employed, and *vice versa*. This is, of course, only to be expected, since capacity and self-induction are inverse functions of each other.

Primary Current.—This may be either from primary batteries, storage batteries, or from a commercial direct-current circuit. Primary batteries have too high resistance to give sufficient current for coils of much size—say 4 inches and over—unless joined in parallel, and then we need a number of such groups to secure the requisite E.M.F. They require constant attention, soon run down, and are expensive to operate. Storage batteries are exceedingly satisfactory, are comparatively inexpensive to operate, and require practically no attention. Charging them is more or less of an annoyance, however, especially where they must be sent

out of the building, as they usually must, besides throwing the entire apparatus out of use while the charging is going on, unless one have a reserve set of batteries.

The utilization of a direct commercial circuit, such as the Edison 110-volt (*e. g.*, where such is available), is, in every way, the most convenient and satisfactory. Until Professor Roentgen's discovery, no method of operating large coils upon such a circuit had been developed. Recently, however, several methods have been brought out, notably that of the

FIG. 4.

"air break," with air blast to blow out the spark, as used by Dr. Wm. J. Morton, Dr. M. I. Pupin, and others. In *Fig. 4* we show a form of apparatus devised by us to accomplish this purpose. A Lundell $\frac{1}{12}$ horse-power motor is supported upon a base with its shaft vertical. A casting, attached to the motor, supports above the latter a copper can made up of two concentric cylinders joined by a ring below, so that the shaft passes up through the inner cylinder and thus avoids the necessity of a stuffing-box. From the shaft is

hung, well within the can, a heavy brass wheel, having stretches of insulating slate let into its periphery. A hard-rubber lid fits over the top of the can. Mounted upon this lid are two brushes, one bearing against the wheel periphery and one against the shaft; suitable springs and screws allow the brush tension to be varied. From the commercial circuit leads are brought to these two brushes, the current being first passed through the primary of the coil and a rheostat in series. Before the lid is put in place the can is filled with distilled water; ordinary hydrant water will answer, although its usual impurity soon causes the water to become dirty, besides allowing a certain amount of electrolysis during use. With distilled water the break may be run for a number of hours without change of the water and very little heating takes place. The real function of the water is not so much to drown the spark as to prevent heating, although it does, of course, also assist in quenching the spark. But a condenser is the real spark extinguisher, and this we connect around the break just as we would around an ordinary vibrating break.

The advantages of this break over all forms of vibrating break thus far known, and over other forms of rotary break, are:

(*a*) *Convenience.*—Mere throwing of a switch being all that is required to start and stop the apparatus.

(*b*) *Noiselessness.*—Only the drowsy hum of the motor being heard.

(*c*) *Reliability.*—No chance of sticking, as with vibrating breaks.

(*d*) *Variable Rate of Break.*—Secured by adjusting rheostat in base of motor.*

(*e*) *Smooth, Unvarying Fluoroscopic Images* by reason of the large number of breaks per unit of time.

Another great advantage of the rotary break is the almost perfect control which it gives over the spark length.

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* We have found that the vacuum of a tube often gets into such a condition as to make the tube absolutely unworkable at a given frequency of break, whereas, a slight *change* of frequency immediately restores the X-radiation.

This is on account of the independence of the coil and the break, thus permitting the breaking of any desired value of current without the slightest change in either frequency or character of break. With the usual vibrating break, operated by the core of the coil, the character and frequency of break is necessarily a function of the value of current, being broken and changed with it. It is impossible, with a large coil, *e. g.*, to get uniform and continuous secondary discharges of a length very small compared with the maximum capacity of the coil, since to such short sparks would correspond a magnetization of the core insufficient to operate the vibrator at all. But with the rotary break we may make the current as small as we please, without in the slightest degree interfering with the frequency and precision of the break.

Two milled heads at top of can permit its lid (carrying brushes) to be withdrawn. A similar head at top of shaft loosens break-wheel, and two heads at bottom of can allow it to be lifted away for cleaning and renewal and replenishing of water.

We use three makes and three breaks per revolution and find best results at from 1,200 to 1,400 revolutions per minute (equal to 3,600 to 4,800 interruptions). We may secure as many as 2,000 revolutions (6,000 interruptions) per minute, by cutting out the motor regulator.

In operation, the brush bearing upon break-wheel should be *positive*, so as to prevent any electrolysis of the break-wheel. We have often broken between 15 and 20 ampères through this wheel for a considerable length of time without any overheating or excessive wear of the wheel. Should the wheel wear in time to an undesirable degree it may be removed and turned down. Provision is also made for replacing the brushes when desired.

General Considerations Regarding Break Frequency.—The question as to whether there is a certain best frequency of break for a given coil or tube or both, has puzzled not a few. Very little has been experimentally determined regarding this point. It is certain, however, that, with a given coil or tube, shortness of exposure is *not* exactly inversely as the

number of breaks. Indeed, it is often far from being so. Some of the best X-ray pictures that have been taken have been by the use of the old "hammer" break of Ritchie, and 4 or 5 clicks of the break have sufficed. Using the same tube and coil, but a rapidly vibrating or rotary break, required 40 or 50 breaks to accomplish the same results. It would appear, hence, that the energy of X-ray radiation *per break* is a function of the number of breaks per unit of time.

We are inclined to believe that this is a matter purely of the "time constant" of the coil. The iron of the core requires a certain time to magnetize and demagnetize. With the breaks few, ample time is given for this process, but when the breaks become rapid this is no longer possible, so that the secondary E.M.F. falls—this may easily be verified by observing the maximum spark length of any coil—with single breaks, few and far between, we get much longer spark maxima than if the breaks are frequent. The thickness of the sparks is also greatly increased. If, however, we increase the *primary* E.M.F. for the more rapid break we may bring up the secondary spark length to the value given by the single breaks. We should thus be able to shorten the exposure as much as we please, by merely increasing the rate of break and the primary E.M.F. at the same time; but here we are limited by the *tube* which cannot *dissipate* more than a certain definite amount of energy in a given time and will break down if overdriven. We see, therefore, that whether we use slow breaks or fast breaks, the actual time of exposure remains practically the same, the few breaks requiring to be distributed over the same *time* as the much larger number of rapid breaks. All this, of course, applies to the photographic plate. For fluoroscopy we require rapid breaks in order to escape the otherwise distressing flickering.

In the above connection, particular attention is directed to one point—the rate of break *must not be too great*. If it is, the iron no longer has time to magnetize and demagnetize. The result is an *alternating* secondary discharge instead of the *direct* discharge desired, and this means blackening of

the tube, *rapid fluctuations of the vacuum, and general deterioration of the tube. We doubt whether, with any coil of size suitable for X-ray work, a rate of break greater than 5,000 per minute should ever be employed.*

Tesla Coil or Induction Coil—Which?—Having carefully examined the results, apparatus and methods of others all over the country, devotees some of the "high-frequency" coil, some of the induction coil, and having made many and careful experiments with both forms of apparatus ourselves, we favor the induction coil. Our reasons for this preference are briefly stated:

(*a*) *Simplicity.*—One coil instead of two.

(*b*) *Ease of Manipulation.*—There being more than double the number of factors to attend to in the Tesla coil than in the induction coil.

(*c*) *Cleanliness.*—The "high-frequency" coil requires an oil bath, which must be renewed from time to time to avoid gumming. It is difficult, also, to find an oil which will not, eventually, act upon the insulation of the wire by virtue of the acid or other impurities which the former may contain.

(*d*) *Sharp Definition.*—The "high-frequency" coil produces an alternating discharge - thus with the double focus tube giving us *two* sources of X-radiation and consequent blurring of the image. If a single focus tube be used, the discharge in one direction is either lost or tears off particles of the platinum reflector, thus blackening the tube and soon destroying its effectiveness.

(*e*) *Noiselessness.*—This, from the surgeon's and physician's standpoint, is, perhaps, the most important considera-ation. With tube in action, the induction coil is practically noiseless, save for the low hum of motor or vibrator. Improperly designed vibrators often rattle and rasp in a very irritating manner. In the "high-frequency" coil, however, the disruptive discharge over the air gap is the vital cause of its action. It is much more violent than a discharge of corresponding length from the induction coil, being of a rattling, cracking character. That such noise cannot but have a most unpleasant effect upon the patient who comes

into the operating room in a condition of more or less nervous collapse to begin with is obvious.

(*f*) *Strain on Tube.*—It has been argued by advocates of the "high-frequency" coil that it is much less hard upon the tubes. Our own observation has been just the reverse of this, and has satisfied us that the wear and tear is very much less with the induction coil.

(*g*) *Results.*—Although we have diligently examined, we have yet to see results obtained by any form of apparatus superior to those made in Philadelphia by Dr. Goodspeed, of the University of Pennsylvania; Dr. Stern, of the Polyclinic Hospital; ourselves, and many others who might be mentioned, and this either as regards detail, penetration or quickness of exposure. Indeed, we may say that we have never seen results obtained with the Tesla coil that are equal in any of the above respects to the results just referred to.

The Tube.—Many forms of tubes have been suggested. With practically no exception, all tubes now in use employ as cathode a concave aluminum disc, whose center of curvature is at the center of a small platinum plate ($\frac{3}{8}$ to $\frac{3}{4}$ square inch in area) the plane of the plate being inclined to the normal from center of the concave cathode. This platinum plate is sometimes made the anode—sometimes the anode is a separate plate or wire elsewhere in the tube. This form of tube is known as the "focus" tube, the cathode rays being focussed upon the platinum plate, which then becomes the active source of X-rays. In the double-focus tube we have two concave cathodes at opposite ends of the tube, the platinum wire reflector being in the form of a wedge with a side presented to each of the concave cathodes.

In the use of the tube everything depends upon the vacuum, X-rays seeming to be of a heterogeneous character, just as are light rays, their quality varying with the degree of vacuum. The higher the vacuum the more penetrating power the tube has, hence it is well to select tubes and use different tubes for different purposes. A tube of extremely high exhaustion is best for working through the body, but with forearm, hands, feet, etc., such a tube has

too great penetration, the bones appearing almost equally transparent with the flesh.

The chief difficulty with most tubes is the change of vacuum which takes place in use. This is thought to be due to the occlusion of the residual gas upon the inner surface of the glass—resulting in an *increased* vacuum; advocates of the bombardment theory believe it to mean an actual driving of the gas out through the body of the tube. It may be partially restored by heating the tube by use of a spirit lamp or Bunsen flame. This must be very carefully done, however, to avoid cracking the tube. When the platinum reflector is

FIG. 5.

used simply as an anti-cathode, and the anode is aluminum, a simple reversal of the discharge will prove fairly successful. Some makers have adopted the scheme, practised now for many years, of blowing a small side pocket upon the tube, into which is placed phosphoric anhydride or some other chemical substance or composition absorbing moisture. When heated, this substance gives off vapor to make up for the loss; it is difficult, however, to drive off just the right amount of vapor in this way, and the vacuum is liable to be made too low, in which case flesh and bone are about equally opaque. So rapid is this change of vacuum with the majority of tubes, that in most cases involv-

ing over a few minutes' exposure or running of the tube one of the above methods becomes necessary. In any case, eventual, almost complete, exhaustion seems to be the rule, and the tube must then be returned to the makers and re-exhausted.

"Blackening" is another cause of decreased efficiency in the tube. It is due to deposition of platinum upon the inner surface of the tube.

The "Bowdoin" tube, shown in *Fig. 5*, was devised after a considerable amount of experimental work, by Profs. Robinson and Hutchins, of Bowdoin College. In general design it does not differ essentially from many other forms of focus tube upon the market. The two respects in which the tube does differ from other tubes are, first, in the arrangement of electrodes, the reflector plate not being connected to the coil at all, but merely acting as an obstacle to the bombarding rays, while the anode is a disc at the end of the tube away from the cathode. This arrangement is found to perfectly and entirely prevent blackening, no matter how long the tube may be used.

The most important feature of this tube is, however, the presence upon its interior surface of a fluorescent material which has been fused into the surface of the glass.* As the tube is used, this material, either by the bombardment or heating, or both, gives off just enough air or vapor to make up for the occlusion due to the natural working of the tube. The vacuum, therefore, tends to remain always constant. Although it seems almost absurd that one could put in just enough of this material to make up for the gas otherwise lost, yet it is a fact that these tubes remain practically unchanged in vacuum for a very long period.

In use the tube requires no "nursing" whatever. It should always have, to start with, a parallel spark gap, which should be quite small, say 1 inch or 1½ inch, and either the primary current or the vibrator tension adjusted so as to

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* The fluorescent quality is not supposed to have any effect upon the result; it merely *happens* that the particular substance found most effective is fluorescent.

produce only a very small secondary discharge. The energy of this secondary discharge may then be immediately increased until the reflector plate shows a dull red spot of about half the size of the nail of the little finger. The tube is now in its most efficient condition, and, with a well constructed and smooth working break, may be run for an hour or two without the slightest attention. The "getting ready," as above described, takes considerably less time than the time it has taken us to describe it. Indeed, in our own work, we do not trouble to make this preliminary adjustment at all, as we make it once for all when we first begin to work with a tube and do not afterward disturb it, merely closing down the switch when starting work.

Life of Tubes.—The life of tubes in general is limited—

(*a*) By time required for the vacuum to become too great, hence requiring re-exhaustion.

(*b*) By ability to stand the electrical strains.

(*a*) Applies to all tubes, so far as we know, save the Bowdoin tubes—probably six or eight hours' continuous working, or the equivalent suffices to incapacitate the average tube. We think that even with the Bowdoin tube there is an evident tendency to eventual very high vacuum, but the time required is quite large, relatively, say ten to fifteen times that operative in the case of other types of tubes.

(*b*) All well-made tubes (from the glass-blower's standpoint) are about equally liable to this limitation. It is probable that these strains are cumulative in their effects upon the tube, the latter gradually weakening until eventually breakdown takes place with secondary discharges much less than would have been originally required for breakdown.

The Stand.—The stand, shown in *Fig. 6*, has been devised by us for applied X-ray work. With it the tube may be placed in any position within a cylinder 6 feet high and 6 feet in diameter, thus making it possible to get under or over a patient, no matter what the latter's position. Four rods (two are not shown in the figure) of vulcanized fibre clamp to the stand in any position and carry the lead wires of the tube, thus keeping them away from one another and

from the metal of the stand. They carry little spring clips at their extremities, by which the wires are caught without tying or bending.* The whole stand is of bicycle tubing and very rigid.

The Subject.—The patient should, of course, be placed in as easy a position as possible. For body pictures, hip and knee joints, etc., a recumbent position may be assumed. The plate should lie beneath and upon a stiff board backing to avoid risk of breakage. Work upon the shoulder, neck, head, etc., may be done with the patient straddling a straight-backed chair, facing the back, and leaning the body against the back for support.† The plate may be bound fast to the body by bandages.

FIG. 6.

Laws for Exposure.—Since the X-rays proceed in straight lines from (approximately) a point source, the intensity of their action varies inversely as the square of the distance from the source. Hence, the *time of exposure* should vary directly as the *thickness* of the intervening substance and the square of the distance from the plate. To

* This is also a great convenience in changing the position of the stand after everything is connected up—as must often be done. The wires simply slip out of the clips, thus relieving the tube of all strain.

† For this suggestion we are indebted to the practice of Dr. Max Stern, of the Philadelphia Polyclinic.

apply these principles in practice, take a trial picture of the hand at 5 inches distance, this being amply sufficient for good definition with such small bones. Say one minute is required for a good result. Then to take a hip joint, we estimate the thickness of the latter to be, say, ten times that of the hand. We must, therefore, expose ten minutes on account of thickness alone. But, owing to the greater distance of bones from plate, the tube must be removed much further to get definition. At least 15 inches from the dry plate should be given—this is three times the distance used for the hand from which the *times* require to be as the squares of these figures, or as one is to nine. The *total* exposure must, therefore, be ninety minutes. This method gives something to go upon. After a little experience, perhaps the most convenient thing to do is to arrange a little table, based upon previous results, and showing times and distances required for well-defined results for distinctive parts of the body.

Short Exposures.—We have obtained and do regularly obtain without difficulty well-defined pictures, showing complete detail of the osseous structure involved in times and with distances from dry-plate to reflector, as below:

Hand and wrist	5	to	10	seconds,	at	5	inches.
Forearm	10	"	15	"	"	5	"
Arm above elbow	½	"	1	minute,	"	7	"
Shoulder	10	"	15	minutes,	"	10	"
Thorax	15	"	30	"	"	10	"
Hip joint	30	"	45	"	"	12 to 15	inches.
Stones in kidneys	30	"	45	"	"	12 " 15	"
Glass, iron, lead, etc., in any part of trunk	30 minutes on an average, at 12 to 15 inches.						

We have very carefully investigated every claim to quicker exposures than these, often by a personal visit, sometimes by correspondence with personal friends whose reliability was undoubted. We have been unable to learn of any results equally good having been obtained in shorter times by any one at any place with any form of apparatus, and we do not believe any such have been obtained.

Manipulation—Development of the Plate.—Suitable and well-constructed apparatus is not the only essential to good results. A great deal depends upon the operator, and com-

paratively trivial differences in procedure make all the difference between success and failure. We describe here the method which we follow and which allows us to count with certainty upon securing at least nine successful plates for every ten exposures.

General Adjustment.—First see that the coil is working smoothly and so as to give a uniform discharge in the primary. The secondary spark points should be separated by the distance given by the makers as proper for the tube. The coil must then be adjusted either by changing the E.M.F. at the primary or by variation of the vibrator adjusting springs, so as just *not* to spark over this air gap. The tube should then be joined in parallel with this secondary spark gap. The coil may then be started up, using the fluoroscope to show whether direction of discharge is right. If not, the primary current should be reversed. If X-rays are not now profuse, the secondary discharge points should be further separated and the vibrator springs tightened so as to produce correspondingly greater discharge. Continue this until either X-rays are secured as desired, or the tube becomes too hot, or possibly bluish or pink in color. Blue denotes too low a vacuum to expect X-rays, but often a gentle running of the tube for ten or fifteen minutes, while in this condition, will raise the vacuum to a suitable value. Pink denotes a *very* low vacuum and usually means, in a tube which has ever *been* right, a puncture or leak; *such a tube* can only be made good by re-exhaustion and repair. With many tubes a vivid green fluorescence is the sign of the X-ray vacuum. With the Bowdoin tubes, as also with many others, the most efficient condition is with the platinum reflector plate at cherry-red heat over an area of one-half or two-thirds that of the little finger nail.

Sometimes the vacuum will be too high for the production of X-rays with *any* length of secondary discharge. It may be reduced by gentle warming of the tube by a spirit-lamp flame. Sometimes reversal of the current in the tube will answer, but this is apt to blacken.

Sticking of the Vibrator must be guarded against—*it is often fatal to the tube.* This is because of the much larger

current through the primary when such sticking occurs. Upon release, therefore, a secondary discharge much above normal is produced, and this the parallel spark gap is unable to carry off. The tube, therefore, receives much more energy than it should, and may break down even though the parallel spark gap be as advised by the manufacturers.

(*b*) *The Plate and Its Development.*—We have used at one time and another a number of different makes of plates with very fair success. For some time past we have confined ourselves to the Carbutt X-ray plates, and though we are not prepared to say that they are the best plates to use, the results are so uniformly good as to make it seem advisable to us to try other plates not radically different in principle.

For developer, we use the Carbutt "J. C. tabloids"—3 J and 3 C, to 5 ounces of water—*add about 2 drachms of restrainer* (the usual 10 per cent. solution). Heating this developer to about 65° or 70° F. just before using, we also find to have beneficial results.

From 15 to 20 minutes will be required for development. The image will come up very slowly and not very sharply. Not much detail will be visible by holding up to the light. Continue development until the general main outlines have appeared and faded away to a general blackness upon the glass side. Then rinse and hypo.

The fixing generally requires from a half hour to two or three hours. This long time is probably *partly* due to the unusual thickness of the Carbutt X-ray films, but probably even more to some chemical condition set up by the unusually large quantities of restrainer used. Mr. Carbutt advises *3* J and *3* C tabloids to *6* ounces of water to *20-30 drops restrainer*. Our experience is that a plate so developed *always requires intensification*; a tedious nuisance, which can never give as good results as a normally exposed and developed plate.

Fluoroscopy.—As a really efficient aid to investigation or to surgical practice, the fluoroscope has thus far proved rather disappointing. In case of very definite fractures of arm or of leg below the knee, as also in certain cases of gunshot wound in these same parts, the fluoroscope will afford definite knowl-

edge. But in all troubles affecting the trunk, upper leg, etc., the indications are too blurred and vague to be relied upon. A good part of this indefiniteness is doubtless due to the *phosphorescent* quality of the screen, which causes images to be retained for an appreciable time after they are formed, so that unsteady holding produces superposed images, while we also have a molecular disturbance extending in every direction in the plane of the screen, producing visibility where no direct X-ray disturbance has been produced. Part of the difficulty is also probably due to the natural inefficiency of the screen, which, as employed in this country, is merely of tungstate of calcium. The platinum salts, especially the platinum barium cyanide, is much superior to the tungstate of calcium, but considerably more expensive; it is generally used abroad. The results obtained with it are much more brilliant than those gotten with the tungstate. It is probable that there are other salts or combinations of salts still more powerful than the platinum.

In using the fluoroscope the room should be made as dark as possible, as the eye thus becomes many times as sensitive. Good tubes are often condemned on account of lack of appreciation of this fact and because they do not reveal much detail when used in a lighted room. We have often seen tubes which would not reveal even the bones of the hand in a lighted room, which, after keeping the eyes in the dark for five or ten minutes, would give very clear fluoroscopic images of the ribs, spinal vertebræ, heart, etc.

It does not follow at all that tubes giving poor fluoroscopic results will also give poor photographic results, or *vice versa*, although usually there is a certain correspondence between the two ideas.

General Considerations.—Speaking mainly from the surgeon's point of view, only experience can insure good results with X-ray apparatus. No matter how simple the apparatus may be, how many safeguards be embodied in it, how explicit and full the directions, there are still a vast number of little points which no book can teach, no tongue tell, and which must be learned. To do the best work, one must be something of a physicist, a photographer, surgeon and doc-

tor, all in one. If he *is* not he must *learn* to be by occasionally injuring his apparatus, and certainly by breaking many tubes.

Particularly must the practitioner be cautioned against expecting too much. Many come to their fluoroscope or X-ray photograph for the first time, expecting to see the whole structure of the body exposed before them as if upon a painted wall chart. This is never the case. Except in the case of the hand, foot or forearm, but little could ever be made out in the fluoroscope were one unaware of what *should* be seen. The photographic plate results are usually better and more definite, but even here interpretation is often difficult.

Stated Meeting, January 26, 1897.

AN IMPROVED AUTOMATIC INTERRUPTER FOR INDUCTION COILS.

BY H. LYMAN SAYEN AND ELMER G. WILLYOUNG.

For a number of months we have been engaged in the systematic development of induction coils, with particular reference to their use in practical X-ray work. The requirements which must be met in such apparatus are vastly more severe than those which have belonged to induction coils as heretofore made. Up to the time of Prof. Roentgen's discovery, coils have been used only for occasional demonstration in academic class-rooms and occasionally for certain scientific investigations, as, for example, in spectroscopic analysis. Under these conditions a great many inconveniences could be put up with without difficulty. The strains put upon the coils lasted but for a short time, and occasional sticking of the vibrator or excessive burning of the contact points during the brief period of the coil's action were permissible. Now, when coils must be run for an hour or two continuously, and, further, must be run by a class of users whose training in physics and the use of physical apparatus is exceedingly meagre, it is obvious that all these

small details must be greatly perfected. A number of the improvements which we have made, we have presented to you in previous papers. We wish to-night to bring before you a new form of automatic interrupter which possesses, in our opinion, many advantages over other forms of interrupters, and which in some respects, we believe, is unique.

In *Fig. 1* we illustrate in diagram the well-known Apps vibrator, which is, perhaps, as largely used all over the world as any form of vibrator thus far devised, and which in its construction and operation is typical of the majority of known forms of vibrators. In action, the hammer-head

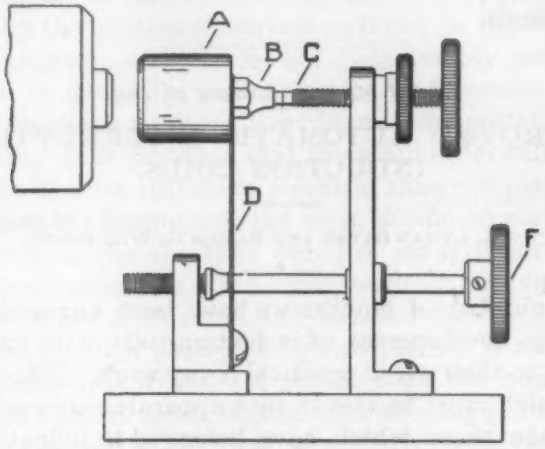

FIG. 1.

(*A*) is attracted by the iron core, thus breaking the contacts (*B* and *C*). The head (*A*) immediately flies back by virtue of the resiliency of the spring (*D*), thus again closing the contacts (*B* and *C*) and allowing the process to be repeated. The tension of the spring may be varied by means of the lower adjusting screw (*F*), which, however, it will be observed, applies the force in a manner mechanically undesirable. But the chief defect in this type of vibrator is the defective principle involved. We see that the circuit is closed by the coming together of the points (*B* and *C*). In breaking the circuit, we require, theoretically, great suddenness, but this we are obviously unable to get since the

hammer-head is at rest and requires a very appreciable time to attain any great velocity of movement, while the current is reduced nearly to its zero value by the first infinitesimal movement of the contact point, thus correspondingly reducing the magnetic pull. Furthermore, the violent blow given to the one contact point by the other throws the spring into a condition of forced vibration, which entails consequent irregular breaking and corresponding irregular discharge in the secondary.

Turning now to *Fig. 2*, we see that the plan of interrupter is entirely different from that just described. The

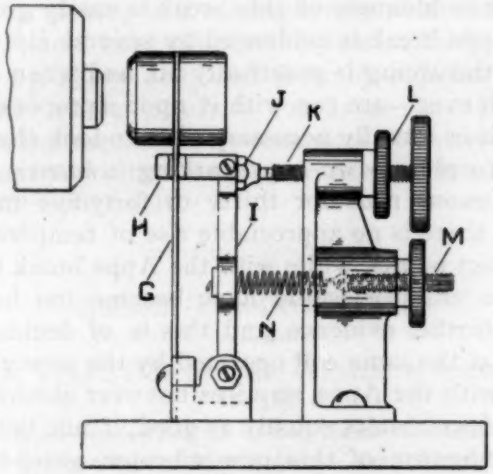

FIG. 2.

spring (*G*) is supported at its lower extremity, just as with the Apps vibrator, but is otherwise entirely free. Surrounding the spring, near the hammer-head, is a yoke (*H*), rigidly attached to a stiff casting (*I*), which is itself pivoted below. This casting carries the front contact point (J). The back contact point (*K*) is controlled by a pair of set-screws (*L*), while a lower adjusting screw (*M*) controls the tension of a helical spring (*N*), itself attached to the casting (*I*). The spring (*G*) is very stiff and when vertical stands entirely free of the yoke (*N*). In action the hammer-head is attracted by the core, but, quite otherwise than in the

Apps interrupter, is able to move a considerable distance and attain a very high velocity of motion before striking the yoke, and thus breaking the circuit. The magnetic pull on the hammer-head being thus released, the hammer-head flies back, partly by virtue of its own spring (G) and partly on account of the pull of the spring (N). The contact points coming together close the circuit once more, and the core's magnetism immediately begins again to act upon the hammer-head, the motion of which, however, it cannot instantly stop, and the head swings on, being gradually brought to rest and its motion reversed again, striking the yoke while moving at high speed.

That the suddenness of this break is vastly greater than with the Apps break is evidenced by several facts. In the first place, the arcing is practically nil, and when coils—say 8- to 12-inch even—are run with it upon a proper electromotive force, it is actually necessary often to look closely at the vibrator in order to *see* any sparking *whatever*. Further, after continuous runs for thirty or forty-five minutes, or even more, there is no appreciable rise of temperature even at the contact points, while with the Apps break the entire mass of the vibrator would have become too hot for the touch. A further evidence (and this is of decided importance) is that the same coil operated by the new vibrator, as compared with the Apps, requires not over one-half the current to produce results equally as good, if not better.*

Another feature of this new vibrator, gained for it by the characteristics which we have just brought out, is that it is capable of direct use upon a commercial circuit of 110 volts, requiring only a rheostat in series with primary to hold the current down to a suitable value. The sparking on such a circuit is greater than with battery, though not so great as with the majority of the vibrators now used with various coils when run by battery.

It is clear that with this form of vibrator we also get

* In an actual test of a 12-inch coil, while giving 10-inch sparks, a 2-inch piece of No. 32 copper (B. & S.) wire was inserted into the primary circuit and grasped between the thumb and forefinger. The heating was not too great to be easily endurable.

rather longer makes than with the Apps vibrator. To change the speed of the vibrator we have only to screw a vertical rod into the hammer-head and upon it place an adjustable bob.

Workers with induction coils have probably noticed the irregular secondary discharge often produced where the vibrator castings were weak or insecurely fastened to the base. This is due to the parts getting in to a condition of forced and independent vibration, partly due to the shocks sustained by the contact points, and partly to the vibrations produced in the coil and reinforced by the resonance of its base. In our vibrator we have successfully overcome this difficulty by making our castings very stiff and mounting the parts upon a hard rubber base, which base we carefully surround by heavy felt, and then by a brass box, which clamps the whole firmly to the base. In this way the vibrator is not mechanically connected to the rest of the apparatus, and any vibrations transmitted to it are instantly damped out by the felt.

NOTES AND COMMENTS.*

DOMESTIC STATISTICS OF THE ALUMINUM MANUFACTURE IN 1896.

From the annual reviews of the various metal industries for 1896, published by the *Engineering and Mining Journal*, the following data relating to aluminum industry in the United States will interest many of the readers of the *Journal*:

The production of aluminum in the United States during the year 1896 was 1,300,000 pounds (650 short tons), as against 900,000 pounds (450 short tons) in 1895, showing a gain of 400,000 pounds (200 short tons), or 44 per cent. As has been the case for several years past, the entire domestic output came from a single producer, the Pittsburgh Reduction Company, whose plant at Niagara Falls has been enlarged, and has been working at nearly full capacity. The advantages of this location are very great for comparatively cheap electric power, and the company, for this and other reasons, has been in complete control of the domestic market. Bauxite is chiefly used as raw material, the company controlling the Georgia Bauxite Company, which in 1895 leased for a term of years the bauxite deposits on the Barnsley estate,

* From the Secretary's monthly reports.

in Bartow County, Ga., and began shipments in 1895. The mineral is sent to the works of the Pennsylvania Salt Company, at Natrona, Pa., where it is worked up into alumina, and the fluorides of aluminum and sodium used in the reduction process. The metal produced at the Niagara Falls plant is manufactured into sheets, bars, rods, wire, tubes, angles, channels and other structural forms, and into small articles at the Company's original works at New Kensington, Pa.

The consumption is divided on about the same lines as formerly, the larger part of the increased demand going into alloys, while the pure, or nearly pure, metal is mostly made up into such small articles as household utensils, implements, instruments, fancy goods, etc. Aluminum bronze and nickel-aluminum are in good favor. The Mitis process of making malleable iron castings with small quantities of aluminum alloy has not apparently fulfilled expectations as to the demand for the metal. For some time the French Government has been interested in the application of aluminum for military and naval purposes, as in the construction of torpedo boats, but no definite information as to results is available. Some aluminum has been used in making bicycles. While thus far the consumption has fairly kept up with the producing capacity of the American and foreign works, it does not appear probable that there is to be any great expansion of business at present prices. For scientific instruments and the usual run of small goods made of pure aluminum, the metal is perhaps cheap enough now, but this field has already been pretty well exploited. For use on a large scale, as for structural purposes in general, it has to compete with cheaper materials. Of course, there are always uses, as for warlike purposes, in which the cost might be a secondary matter in view of the specially advantageous properties of aluminum, but thus far such utilizations have been usually deferred for the future.

The following table shows the production of aluminum in the United States for six years, the figures including the aluminum used in alloys:

	Production.	
Year.	*Pounds.*	*Value.*
1891	168,075	$126,056
1892	295,000	191,750
1893	312,000	202,800
1894	817,600	490,560
1895	900,000	495,000
1896	1,300,000	520,000

From this it appears that the output has been steadily increasing, without setback in any one year, notwithstanding depression in general business; but the values have not quite kept pace with the quantities. For 1896, the average price is taken at 40 cents per pound.

The production in the United States has been somewhat over one-third that of the world. The principal European producer is the Aluminium Industrie Gesellschaft, with works at Neuhausen, Switzerland, and controlling the Société Electro-Metallurgique de France, with works at Froges, in France. In 1895 the Neuhausen works turned out about 650,000 kilograms, and the Froges works about 100,000 kilograms. The British Aluminium Company, using Irish

beauxite, has been making extensive preparations, and will now appear as a producer. The total output of aluminum in the world during 1896 has not yet been reported, but in 1895 it was approximately 1,150 metric tons (2,535,290 pounds, or 1,268 short tons).

Prices.—At the beginning of 1896 the American scale was as follows: No. 1, 98 per cent. pure, ingots for milling, 50 cents to 55 cents; No. 1, ingots for remelting, 48 cents to 53 cents; No. 2, 94 per cent. pure, ingots for remelting, 42 cents to 50 cents, the range in each case depending on quantity. These rates held until July, when we quoted No. 2, 94 per cent. pure, ingots for remelting, at 38 cents to 43 cents, and ingots from scrap at 35 cents to 40 cents; No. grade as before. In November, the Pittsburgh Reduction Company announced a reduction in prices of from 5 cents on the lower grades to 11 cents on the higher, the new schedule (which has been maintained to the close of the year) standing thus: No. 1 (guaranteed over 98 per cent. pure), ingots for remelting, 42 cents in small lots; 39 cents in 100-pound lots; 37 cents in ton lots; No. 2 (guaranteed over 90 per cent. pure, with no injurious impurities), ingots for remelting, 34 cents in small lots; 33 cents in 100-pound lots; 31 cents in ton lots; nickel-aluminum casting metal (pure aluminum alloyed with less than 10 per cent. nickel and other hardening ingredients), 40 cents in small lots; 38 cents in 100-pound lots; 35 cents in ton lots; special casting alloy (containing over 80 per cent. pure aluminum, used in place of brass), 35 cents in small lots; 30 cents in 100-pound lots; 27 cents in ton lots; castings, 45 cents per pound, upward, and special rates for bars, rods, shapes, etc.

ENGINEERING NOTES.

The *first use of Niagara's power* was made in 1725, a primitive sawmill being operated. Nothing more was done until 1842, when Augustus Porter conceived the plan of hydraulic canals, and, in 1861, one of them was completed. The Cataract Construction Company, from whose plant power has just been delivered in Buffalo, was incorporated in 1889.

CAR BUILDING IN 1896.

In its annual review of this important industry for the past year the *Railroad Gazette* reports a notable improvement over the figures of 1895, a somewhat unexpected statement.

The *Gazette* says, in the course of its comments:

"We have built, in 1896, 33,000 cars more than the total of two years ago, yet the total is still below the lowest of any year before 1894, for which we have figures that are comparable. These go back to 1888, and the lowest total in that period is in 1893, when 56,900 cars were reported built, or about 6,000 more than in 1896. But one of the large companies, whose output is included this year, did not report in 1893, so that the difference is actually larger. Comparing with 1890, the best year for car building in the last decade (the best for the locomotive builders also), we find that the 1896 output was

not half that of the earlier year, when 103,000 cars were built. In both 1891 and 1892 also the contracting shops turned out within 4,000 of 100,000 cars, so that if the records for those years represent a normal annual addition to the car equipment, the distribution of car orders, which may be looked for with a year or two of good traffic, will put great numbers of people at work. The car-building industry is a far-reaching one, and activity in it sets a long line of trades in motion."

MINERAL AND METAL PRODUCTION OF THE UNITED STATES IN 1896.

The production of minerals and metals in the United States for the year 1896 is estimated by the *Engineering and Mining Journal* at a total value of $653,311,468, showing a decrease, as compared with 1895, of $24,689,266, or about 3½ per cent. This decrease was largely in values rather than in quantities; in none of the chief articles was there any marked decrease, while in several there were considerable increases.

The total production of metals in the year 1896 was valued at $242,311,481, an increase of $1,694,111 over the previous year; while the value of non-metallic substances was $410,999,987, a decrease of $26,383,377 from 1895. A large part of this was due to the lower values of coal, stone and a few other important substances, very little resulting from the smaller quantities.

THE JACQUES CARBON GENERATOR.

Since the publication of the experiments of Dr. Jacques with an apparatus for generating energy directly from coal, the technical journals have devoted much space to the discussion of the merits of his work. The strongest adverse criticisms that have been made in relation thereto, have originated with Mr. C. J. Reed, who has challenged the correctness of the author's explanations as to the origin of the energy developed in the Jacques apparatus, and who has, we think, successfully demonstrated that the phenomena observed are thermo-electric, (deriving their source from the fuel consumed in maintaining the apparatus in action), and not electrolytic.

In a recent article published in *Harper's*, Dr. Jacques makes an interesting exposition of the subject from his standpoint. He announces certain convictions, of which we give an abstract in what follows:

The first is that the electric current was due to the chemical combination of the oxygen of the air with the coke (carbon). Quantitative tests showed that oxygen was taken from the air; that the carbon was consumed; that carbonic acid was formed. At the same time the electromotive force obtained agreed almost exactly with that theoretically obtainable from the combination of oxygen with carbon to form carbonic acid (1·04 volts). That the phenomenon was not due to the thermo-electric action was proved by the fact that when the whole apparatus was so inclosed that all parts were kept of uniform temperature the maximum electromotive force and current were obtained. Some experiments with larger apparatus have confirmed these results, and

have shown that under proper conditions the electrical energy obtainable from one of these generators is substantially equal to the potential energy of the weight of carbon consumed within the pot.

So far, only relatively small carbon electric generators have been built; and the author points out that with this generator, as with the steam engine, increased size means increased efficiency per pound of coal. Some results of a test (made by experts not connected with the development of the invention) upon a small and comparatively crude 2-horse-power carbon electric generator that has been in occasional use for some six months, are as follows: Average electrical horse-power developed, 2·16 horse-power; average electrical horse-power used by air-pump, 0·11 horse-power; average net electrical horse-power developed, 2·05 horse-power; carbon consumed in pots per electrical horse-power hour was 0·223 pound; coal consumed on grate per electrical horse-power hour was 0·336 pound; total fuel consumed per electrical horse-power hour, 0·559 pound. The electricity obtained from 1 pound of coal (of which 0·4 pound was consumed in the pots and 0·6 pound was burned on the grate), was 1,339 watt hours, or 32 per cent. of that theoretically obtainable.

These figures, it is claimed, show that the efficiency of this particular generator was twelve times greater than that of the average electric light and power plant in use in this country, and forty times greater than plants of corresponding size. There are, the author says, many details still to be worked out, and many improvements yet to be made, before the carbon electric generator can be put into general commercial use on a scale comparable with that of modern steam engines.

THE LATEST FEAT IN ARCTIC EXPLORATION.

The year of 1896 was signalized by the finding of Dr. Nansen by the Jackson party in the Arctic regions, and the announcement that on April 7, 1895, he had succeeded in penetrating northward to latitude 86·15—a point nearly 200 miles nearer the North Pole than any preceding explorer had reached. Hitherto the highest latitude attained had been 83·24, reached by Lockwood and Brainard, of the Greely expedition, in 1882. Nansen approached to about 225 miles from the pole, and he gives it as his opinion that nothing but lack of dogs and kayaks prevented him from covering the intervening distance.

His achievement opens up a new chapter in the history of Arctic exploration. Without the loss of his ship *Fram*, without any serious mishap whatsoever, even without serious discomfort, he succeeded in subtracting nearly 200 miles from the distance separating man's actual attainment from the final goal of Arctic search. It had taken about 300 years, with enormous expenditures of money, ships and human lives, and at the cost of untold suffering, for the accumulated labors of his predecessors to make an equivalent northward advance. Professor S. A. Andree's sensational attempt to reach the North Pole by means of a specially-constructed balloon closed unsuccessfully. The professor, with two companions, sailed from Gothenburg, Sweden, June 7th, on the *Virgo*, and took up quarters at Danes Island, Spitzbergen. Ad-

vices received at Hammerfest, Norway, August 6th, stated that Professor Andree had completed the inflation of his balloon and was awaiting a favorable wind. But on August 24th the *Virgo* arrived at Tromso, bringing back the expedition, which had abandoned its trip for this year owing to the lateness of the season.

Franklin Institute.

[*Proceedings of the stated meeting, held Wednesday, February 17, 1897.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 17, 1897.

JOHN BIRKINBINE, President, in the chair.

Present, 165 members and visitors.

President-elect Birkinbine, in assuming the chair, made a brief address appropriate to the occasion.

Additions to membership since last report, 26.

The Actuary transmitted from the Board of Managers a memorial of the late Chas. H. Banes. The Secretary read the memorial, which was accepted and ordered to be referred to the Committee on Publications.

(This publication will be made in the impression of the *Journal* for April, 1897.)

A communication was read from Mr. W. P. Tatham, tendering his resignation as Vice-President of the Institute. The resignation was accepted.

An election was held to fill the vacancy thus created, which resulted in the choice of Mr. Alfred C. Harrison.

A communication from Mr. Jos. M. Wilson, the retiring President, was read, conveying his acknowledgment of the resolution of the Institute, passed at the annual meeting of January 20, 1897, in reference to his retirement.

Mr. Thomas Corscaden, of the American Pulley Company, of Philadelphia, gave a description of the "All-Wrought-Steel Belt Pulley" of his invention, manufactured by the company above named. The speaker illustrated the subject by the exhibition of a specimen pulley, and of the parts of which it is composed.

Mr. John Carbutt, by special request, gave an experimental demonstration of the apparatus and operative methods employed in X-ray photography.

The Secretary read a communication from the President of the Association of American Draughtsmen, inviting the members and friends of the Institute to attend a lecture on "Aërial Navigation," by Mr. Wm. N. Howell, to be given in the Hall of the Institute, on Thursday, February 18, 1897, at 8 o'clock.

Adjourned.

WM. H. WAHL, *Secretary*